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THE ETIOLOGY OF CATARRH.

Inaugural Address

BY

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IN no branch of medical science is greater activity to be observed in the present day than in the investigation of the causes of disease, and in none is there larger promise of benefit to mankind. Successful modes of treatment may be suggested by analogy and confirmed by experiment, even where the nature of the disease is but dimly discerned, and its essential cause unknown. But it can scarcely be doubted that a more exact knowledge upon these points will sooner or later lead to important results. Not therapeutics only, but hygiene also, must be greatly helped by the development of sound views on the causation of disease. From the practical point of view, there-

fore, no less than from the purely scientific, one cannot but regard very hopefully the researches of Koch and others on the part played by microscopic organisms in the production of disease. We may differ among ourselves in our readiness to accept new doctrines (and it is wise not to be hasty); but no one, I think, can fail to see that year by year our knowledge in this direction is being extended and consolidated. Nor is it only by the labours of microscopists that these results are achieved. The volume lately issued by the Collective Investigation Committee of the British Medical Association supplies us with a large body of evidence pointing to the conclusion that acute pneumonia is, in some cases at least, a specific disease, depending therefore, in all probability, like other specific diseases, upon the presence of a microscopic organism.

The subject of my address this evening is not without relation to these inquiries. I propose to ask your attention to certain views which I have long held on the Etiology of Catarrh.

The most widely prevalent idea regarding the origin of catarrh is that which finds expression in the term by which in this country we commonly designate the disorder. But the term "cold" can scarcely be said to cover all the various agencies to which catarrhs are popularly attributed. One individual catches cold, as he believes, from walking in his garden without his hat, although the air to which he is exposed may be anything but cold. Another takes the complaint from sitting in a current of air between a window and a door, although the movement of air in the room may be trifling compared with that which out of doors he would consider healthful. A change from winter to summer clothing is

generally considered to be attended with danger. Some people will take cold through reading a newspaper fresh from the press, and I suppose we have all met with nervous patients who have been rendered uneasy about their own health by our entering their apartment with damp clothes.

But leaving the vagaries of hypochondriacs, we find in authors of repute a striking consensus of opinion on the action of cold as the exciting cause of catarrh.

In Cullen's *Practice of Physic* (1786) we read: "The remote cause of catarrh is most commonly cold applied to the body." And again: "The application of cold which occasions a catarrh probably operates by diminishing the perspiration usually made by the skin, and which is therefore determined to the mucous membrane of the air-passages. As a part of the weight which the body daily loses by insensible evacuation is owing to an exhalation from the lungs, there is probably a connection between this exhalation and the cutaneous perspiration, so that the one may be increased in proportion as the other is diminished; and therefore we may understand how the diminution of cutaneous perspiration, in consequence of the application of cold, may increase the afflux of fluids to the lungs, and thereby produce a catarrh." Cullen goes on to speak of the epidemic or (as he calls it) contagious catarrh. "There are," he says, "two species of catarrh. One of these, as I suppose, is produced by cold alone, and the other seems manifestly to be produced by a specific contagion. Of such contagious catarrhs, I have pointed out many instances occurring from the fourteenth century down to the present day. In all these instances the phenomena have been much the same; and the disease has always been particularly remarkable in

this, that it has been the most widely and generally spreading epidemic known."

Dr. John Brown, who was a pupil and contemporary of Cullen, and who was given to turning the world upside down with his theories, taught a quite opposite doctrine respecting the origin of catarrh. "Heat alone," he says, "fulness of diet, strong drink, and moderate exercise, for certain produce it; cold, cold water, spare diet, and rest, as certainly and effectually remove it. It was, therefore, a very unlucky mistake to think it arose from cold alone, and was to be cured by heat. The occurrence of catarrh so often in summer, where its action can be a thousand times traced back to heat, but never to cold; the influenza never needing the assistance of cold to induce it, which catarrh often does; its never succeeding to pure cold, but immediately to heat—facts known to old women, to shoemakers and taylors, to blear-eyed beggars and barbers, unknown to medical authors and professors—are all circumstances that confirm the same fact."

But Brown's views left no permanent impression on medical opinion; for, coming down to our own day, I read in Sir Thomas Watson's *Lectures:* "Catarrh is the commonest of all disorders. Not one man in ten thousand passes a winter without having a *cold* of some sort. And this name points to its ordinary cause—cold somehow applied to the body. It does not always or often result, I apprehend, from cold air brought into contact with the membrane itself in the process of breathing; but from cold, and especially from cold and wet, applied to the external integument."

But Watson, like Cullen, recognises that the great epidemics of catarrh cannot owe their origin to cold. "The facts that have now been mentioned," he writes,

"respecting the influenza, warrant, I think, the conclusion that it does *not* depend upon any mutations in the ordinary and obvious qualities of the atmosphere; upon any degrees or variations, I mean, of its temperature, its motions, or its moisture; upon what is expressed in the single word *weather*." Of Schönbein's theory, connecting epidemic catarrh with the presence in the atmosphere of an excessive quantity of ozone, Watson speaks with some favour, regarding it as not unworthy of careful investigation. He continues: "Another hypothesis, more fanciful perhaps, at first sight, than these, yet quite as easily accommodated to the known phenomena of the distemper, attributes it to the presence of innumerable minute substances, endowed with vegetable or with animal life, and developed in unusual abundance under specific states of the atmosphere, in which they float, and by which they are carried hither and thither. Myriads of these animalcules, or of these vegetable germs, coming in contact with the mucous membranes, and especially with that of the air-passages, irritate (it is imagined) those surfaces, and exercise a poisonous influence upon the system. Now the sporules of certain fungi, which ruin the health and destroy the vitality of larger plants, on which they prey, are inconceivably small. I shall prove to you presently that vegetable effluvia are capable of producing, in the human body, symptoms not very dissimilar from those of influenza. Again, that the waters of this globe swarm with living creatures, which are invisible by our unaided eyes, the microscope has taught us. Others, too minute to be estimated even by that wonder-showing instrument, in all probability exist. We cannot doubt that the gaseous fluid which surrounds this planet equally teems with

living atoms. We know that multitudes of insects, and of cryptogamous plants, infinite in number with respect to our finite powers of computation, *are* sometimes suddenly hatched or developed in places which were previously free from them. It is easy to conceive that atmospheric infusoria (so to speak) may rapidly congregate, or vivify, in masses sufficient to render deleterious the very air we breathe. If this be so, we can understand how such a cause of disease may first act here and there, and presently overspread large districts; how it may move, or be wafted, from place to place, or be carried about by persons; how its course and operation may be circumscribed and definite; and how some germs or ova may remain after the visit, retaining their vitality, and ready in future seasons again to start into life and activity under favouring circumstances. Taking the insect hypothesis, and knowing, as we do, that some animal poisons (that of small-pox, for example) have the singular property of multiplying themselves in the human body, like yeast in beer, we may conceive that diseases, produced by animalcules, may thus infect the fluids of the body, and become contagious in the fullest sense of that term."

This zymotic theory, you will observe, is only applied by Watson to the great epidemics of catarrh which from time to time sweep over large tracts of country.

Niemeyer, in his *Text-book*, ascribes ordinary catarrh to "chilling of the external skin, and the action upon it of a sudden change of temperature;" but admits that "we cannot satisfactorily account for the phenomenon." He adds that "in very many cases the exciting causes of catarrh are unknown, unless we accept the explanation with which people usually content themselves, that 'they must have taken cold somewhere.'"

Lastly, to bring my references down to the latest date, I find in Quain's *Dictionary* nothing further respecting the causation of catarrh than the bare statement, that it "commences shortly after exposure to cold, or more particularly to cold and damp."

In the face of authorities such as I have quoted, and in the face (as I suppose) of almost universal medical opinion, it may seem presumptuous in me to question the direct connection between cold and catarrh. Nevertheless, such is my contention; and I can only hope that the inequality of the conditions may not lead my hearers to regard with preconceived disfavour the arguments by which I hope to defend my position.

My first argument will be based upon the similarity—I might say the identity—of the symptoms observed in a common cold with those observed in the wide-spreading epidemics of influenza. The difference appears to me to be one of intensity merely—intensity in respect of the geographical area involved, intensity in respect of the number of persons attacked within a given area, and (following necessarily from this) intensity in respect of the absolute number of severe, dangerous, or fatal cases. I need not dwell upon the points of resemblance. It will be sufficient if I remind you, on the one hand, that by common consent we speak of the severer forms of ordinary catarrh as "influenza colds;" while, on the other hand, it is clear from the published records that in the most serious epidemics of influenza a large proportion of cases have not, even in point of severity, differed from the common run of colds. I will go further, and say that what is known as sporadic catarrh, by way of distinguishing it from the widespread epidemics just referred to, is very often itself epidemic in a minor degree. This

is a matter of common observation, although the lesson to be learned from it has been (as I think) strangely overlooked.*

Primâ facie, then, there is good ground for supposing that influenza-catarrh and common catarrh are one and the same disease, and that any theory of causation which can be proved of the one is equally true of the other. Now it is but little that we know definitely of the exciting cause of influenza; but there is one thing that we know about it with absolute certainty. The recorded history of influenza epidemics positively precludes the supposition that the attacks are due to anything appertaining specially to the individuals attacked. This remark is true of all epidemic diseases, but influenza is an epidemic of epidemics. The observation of Cullen already quoted is as true to-day as it was a hundred years ago. Influenza is beyond doubt "the most widely and generally spreading epidemic known." Of an epidemic in 1557, it is recorded by a contemporary writer that the disease attacked all parts of Spain at once, and that the greatest part of the people of that kingdom were seized with it almost on the same day. Dr. Glass, of Exeter, describing the epidemic of 1775, writes: "On the 11th or 12th of November it made its appearance in the Devon and Exeter Hospital, and within a week seized 173 persons, being all the

* It may be said that the apparently epidemic character of common catarrh admits of being explained by a reference to the weather prevailing at the time when such epidemic character is observed. I do not think this is in any case a satisfactory explanation; but there is a class of cases in which it conspicuously fails. Some years ago I was staying with a party of friends at Torquay. The month was August; the weather fine and summer-like. The party consisted at one time or another of eight persons, and of those eight, five became affected with catarrh. Instances of this kind are not uncommon, and receive no explanation from any of the current theories of catarrh.

servants and patients then in the house, except two children; 162 of them were coughing together." At St. Petersburgh, in 1782, it is related that forty thousand persons were taken ill in one morning with influenza.

Now no one in his senses would suggest in explanation of these phenomena that all these crowds of people in Spain, Exeter, and St. Petersburgh had been walking in the garden without their hats, or sitting in damp boots, or indulging in any of the other indiscretions to which it is customary to attribute sporadic catarrh. Nothing can be more certain than that the exciting cause of influenza (whatever be its precise nature) is to be found in some general influence pervading the entire community; and to my mind it is hardly less clear that common catarrh, essentially identical as it is with influenza, not only in its symptoms, but even in its epidemic tendency, owns a similar origin.

But here crops up a difficulty arising from individual experience. "You may theorise as you please," I hear in imagination some one say, "but experience is better than theory, and you will never persuade me that my cold was not caught last Sunday in church through sitting beneath an open window. I felt the draught, and was certain at the time that I was taking cold."

"Experientia fallax," says the Father of Medicine; and without inquiring too curiously whether Hippocrates meant his words in the sense in which we understand them, I take them as embodying, in that sense, a profoundly wise caution. Experience can only tell the subject of a cold that he is the subject of a cold. It cannot tell him how he contracted it. That is a matter of theory more or less probable, and I think it is not difficult to

show that the theory commonly adopted is exceedingly improbable.

For, in the first place, the cause assigned seems altogether inadequate to the effect produced. There is no one engaged in the ordinary affairs of life who does not continually expose himself to far greater and more sudden changes of temperature than would be involved in sitting by an open window. If the latter is sufficient to induce catarrh, what shall we say to the exposure of the naked body in a cold bath? True, the one is intentional, the other accidental and unsought; but is it rational to suppose that the effect will be determined by circumstances such as these?

Again, there is a source of fallacy in the impression which the subjects of catarrh often have regarding the time and circumstances of their taking cold. In many cases one of the early symptoms of catarrh is a sense of chilliness. This is naturally attributed to external causes, and the individual traces his illness to the particular exposure which he suffered at that time. In truth, the sensation was but the mild representative of the rigor with which febrile disorders are commonly ushered in. Precisely the same error is continually made with regard to more serious diseases. A patient with typhoid fever, or even with small-pox, may indicate the precise occasion on which he took the "chill" which he believes induced his disease, ignorant of the fact that for many days previously the disease had been breeding in his system, and was only then beginning to assert its presence. The initial symptom is mistaken for the exciting cause. This fallacy may go a long way to explain the widespread belief that catarrh is due to cold—a belief which I hold to be an error, but an error almost hopeless of eradica-

tion, seeing that it has struck its roots into our very language.

Another source of fallacy may be mentioned. Exposure to cold air may directly excite sneezing, and as sneezing is a symptom of catarrh, the person affected concludes that he has taken cold; and if the cold should not develop, he will perhaps flatter himself that he has warded it off by prompt recourse to hot foot-baths and gruel. But sneezing, although a symptom of catarrh, is not catarrh, and in the case supposed has nothing to do with catarrh.

If the circumstances to which catarrhs are commonly attributed were circumstances of an exceptional character, it would be right to attach a certain weight to the popular judgment respecting them. But the case is far otherwise. The assumed causes are for the most part occurrences of the most commonplace kind, continually happening without observed consequences, but now and again followed by a cold. *Post hoc, ergo propter hoc.* The many instances in which a draught or a shower is encountered without harm following either pass unnoticed, or, fitting into no theory, are soon forgotten. The few instances in which a cold follows arrest the attention and become impressed upon the memory. The supposed exciting causes being so common as they are, and the complaint attributed to them being so common too, it would be strange if occasional coincidences did not occur, though no causal relation existed between the two.

Dr. Tanner, in his *Practice of Medicine*, while adopting in general the view of other writers with regard to the origin of catarrh, adds the following qualification: "It arises not from mere cold, but from too sudden a change of atmosphere or from exposure to wet, &c., when the strength

is exhausted. . . . The application of cold is only dangerous when the heated body, exhausted by exercise, is rapidly parting with its caloric." Assuming this qualification to be reasonable, I would hang upon it another argument. Of all the persons who are able (as they believe) to refer their colds to a particular exposure, how many, I ask, would be able truthfully to say that at the time of exposure their heated bodies were exhausted by exercise? Surely a very small proportion. But if that be so, it follows that a very large proportion of the colds which are believed to be accounted for are really not accounted for at all; that is to say, the great majority are as yet without any further explanation than that with which, as Niemeyer tells us, people usually content themselves, that "they must have taken cold somewhere." The absence of explanation hitherto must be allowed to be a strong point in favour of an explanation offered.

I have read somewhere a theory of catarrh which has not, I think, found its way into our text-books. According to this view, cold applied to the skin is the exciting cause, but the effect is produced through the agency of the nervous system in a reflex manner. This explanation is plausible, and meets one of the objections that I have raised to the common view—namely, that of disproportion between cause and effect. It is characteristic of reflex nervous phenomena that there is no necessary proportion between the cause and the effect. The touch of a feather may induce a shiver, which the dead weight of a crowbar would fail to excite. But are there (I would ask) any positive observations warranting the idea that the application to the skin of an agent *per se* innocuous is capable of producing in this reflex manner hyperæmia of distant parts? I greatly doubt it. Besides, if the air in

which we live and move, and which is never constant in temperature, were capable, with every trifling change, of setting up internal disease, it is difficult to see how life could be maintained at all.

It might help us to some further knowledge of the etiology of catarrh if statistics were available respecting this very common complaint—too common, apparently, to have been thought worthy of statistical treatment. In the absence of any better material, I may be excused if I fall back upon some records of personal experience. I have been in the habit of noting regularly the colds from which I have myself suffered, and I find that my record extends now over twenty-four years. The total number of colds recorded during that interval is 82, giving an average of about $3\frac{1}{2}$ per annum. The number may seem large; but I should observe that it includes colds of all degrees of severity and duration. Thus, out of the 82, 8 were noted as having a duration of two days only, and 12 a duration of three days. The mean duration of the whole number was nine days, the disorder not being considered at an end so long as any trace remained. If the two-day colds were excluded, as not worthy to be considered developed colds, the number would be reduced to 74, or 3 per annum. But for my present purpose the slight colds are as valuable as the more severe, both alike indicating the presence of an exciting cause. No calendar year in my series was without a cold; but there was once an interval of twelve months, from February to February. The annual totals ranged from one to seven. Judged by Sir Thomas Watson's assertion, that not one man in ten thousand passes a winter without a cold, I must have enjoyed more than an average immunity. Twice have I passed unscathed through what may be called the five

winter months (November — March), and five times through the shorter winter comprised in the three central months of that season (December—February).

But if my statistics are to be relied upon, the winter is not, in point of fact, the season when catarrhs most prevail. The highest of my monthly totals occurs in April; the lowest in July. April furnished no less than 15 colds in the twenty-four years; July only one. The winter months occupy an intermediate position, the numbers ranging from 6 to 9.*

Considering the length of time over which the observations extend, and the large inequality in the monthly totals, and weighing these two circumstances against the admitted disadvantage attaching to results derived from a single person's experience, I am disposed to think it may be possible to deduce from the figures some conclusions which may not be altogether undeserving of trust.

Why should the month of April be specially prolific of catarrh, far more so than either of the winter months? This fact, at all events, does not point to cold as the essential cause; still less does it point to wet, for April is the driest of all the months.

But it will be safer not to rely upon the evidence of single months. By grouping the months in accordance with their meteorological characters, we may look for more reliable results.

And first we will group them in accordance with their mean temperature,† comparing the three coldest months (December, January, and February) with the three

* The following is a complete list of the monthly totals:—January, 9; February, 9; March, 7; April, 15; May, 7; June, 4; July, 1; August, 2; September, 6; October, 10; November, 6; December, 6.

† The meteorological data quoted in this address are all derived from observations made at Clifton.

warmest months (June, July, and August). In the three coldest months I find 24 catarrhs; in the three warmest, only 7. Here, then, is evidence that atmospheric cold, although it may not be the essential cause, is an important factor in determining the occurrence of catarrh.

To discover, if possible, the influence of rainfall, I first of all compare the three driest months (February, March, and April) with the three wettest months (August, September, and October). In the three driest months, with a mean aggregate rainfall of 6·7 inches, I find 31 catarrhs, the largest number in any three months; in the three wettest months, with a mean aggregate rainfall of 10·7 inches, I find only 18 catarrhs. Here we have a prevalence of catarrh coinciding with comparative drought, and a rarity of catarrh coinciding with abundant rainfall. This comparison, however, is less conclusive than the former, because the effect of temperature, if considered to be established by the first comparison, may be held sufficient to explain the difference in the second comparison; for the three wettest months, although not the warmest, are comparatively warm, and the three driest months, although not the coldest, are comparatively cold.

A more instructive comparison may be instituted between the three coldest months (December, January, and February) and the three driest months (February, March, and April). The mean temperature of the first of these two periods is 39·5; that of the second is 42·3. The mean aggregate rainfall of the first is 8·4 inches; that of the second, as already mentioned, is 6·7 inches. Now if cold were the only cause of catarrh, or the only meteorological condition affecting its prevalence, the three coldest months should include a larger number of

attacks than the three driest months; but the reverse is the case, the three driest months including the larger number, in the proportion of 31 to 24. It would therefore appear as if a deficient rainfall were really an effective agency in promoting the prevalence of catarrh.

Pursuing the comparison of the three coldest months with the three months of least rainfall, it may be well to inquire how the two periods are related in respect of atmospheric humidity. The mean humidity of December, January, and February I find to be 87·4 (saturation = 100); the mean humidity of February, March, and April is 83·3. The comparative frequency of catarrh, therefore, in the three last-named months may be either due to deficient rainfall, or to atmospheric dryness, or to both combined. The three driest months of the year in respect of atmospheric humidity are May, June, and July; but the higher temperature of these months overrides and obscures all other influences.

There is still another comparison that may be made between the three coldest months and the three months of least rainfall. I refer to the relative prevalence of north-easterly and south-westerly winds. Calling all winds north-easterly which blow from points between north and east inclusive, and all south-westerly which blow from points between south and west inclusive, I find that while in the three coldest months south-westerly winds prevail over north-easterly in the proportion of 32 to 24, in the three months of least rainfall north-easterly winds prevail over south-westerly in the proportion of 29 to 28. Here, then, is a third point of difference between the two periods now under comparison, and one which may claim, equally with the difference of rainfall and the difference of atmospheric humidity, a share of

influence in bringing about the difference observed in the prevalence of catarrh—a difference which we have seen to be in a direction opposite to that which temperature alone should occasion.

To sum up the results which seem deducible from my figures, I would begin by throwing out two suggestions of a negative character, both in opposition to current opinions. First, if "getting wet" be an influential cause of catarrh, it is difficult to see why catarrhs should be most frequent in the months when rainfall is least. Secondly, if cooling of the body after free perspiration be a fruitful cause, it is strange that the fewest catarrhs should occur in the warmest months.

The positive conclusions at which I arrive are the following:—

First, catarrh is promoted by the cold of winter, and is greatly checked by the heat of summer.

Secondly, it is promoted by one or more of three causes which occur together—namely, deficient rainfall, dryness of air, and north-easterly wind.

But I need hardly point out how unphilosophical it would be to infer from these results that either the cold, or the dryness, or the north-easterly wind is the essential cause of catarrh. Plague, cholera, and yellow fever are all in a marked degree under the influence of atmospheric conditions, notably of temperature; yet no one believes that either of these diseases is the direct product of heat, or hesitates to assign it a place amongst the zymotic group.

I apply the same principles to catarrh. I believe it to have its origin in a specific organism, the germs of which float in the air. This organism, like other organisms, is dependent for its activity and propagation upon atmos-

pheric conditions. Unlike many, it thrives in cold and languishes in heat. Dry north-easterly winds bring to it life and vigour; while south-westerly winds depress its energies, and the rains which accompany them wash it away. Drawn into the air passages with the breath, it lodges in the mucous membrane, where probably it multiplies and spreads, giving rise to the local symptoms of a cold. There may be nothing more than this, or the organism, penetrating the tissues, may find its way into the blood, and, by its presence in the circulating fluid (where again it multiplies), may set up a state of fever.

To conclude, if the subject of my discourse seem comparatively trivial, I may be allowed to plead that in science nothing is trivial; and that as no error can fail to be hurtful, so no truth can fail to be helpful, and that in a degree not to be measured by the intrinsic importance of the subject to which the truth or the error immediately refers.

SOME POINTS IN THE PATHOLOGY OF GENERAL PARALYSIS OF THE INSANE.

BY

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THE discovery of the important diagnostic significance of the tendon-reflexes has enabled us to review with profit our knowledge of many diseases of the nervous system, amongst them General Paralysis of the Insane. Owing, probably, to the interesting combination of mental and physical signs, this disease has always possessed special attractions in the eyes of alienists, and many are the knights in pathology who have broken a lance over it; notwithstanding, its pathology is still obscure, and, for peculiar reasons, likely to remain so for some time. One difficulty lies in the fact that opportunity of examination of the nervous centres generally arrives only when the morbid process has done its worst, the quantity of structural mischief then disguising the quality. On the other hand, we are, I believe, almost entirely ignorant of the first expressions of the disease, a distinct feature of which is subtlety of advent. As yet its earliest recognition depends upon signs which point already to serious structural defects initiated, probably, some considerable time previously. I will, indeed, suggest that in cases of ordinary duration *after* diagnosis, the morbid process has existed for at least as long a period *before* diagnosis—the diagnosis resting upon such signs as pupillary changes,

slight defects of articulation, moral slidings (often most early observed), &c. In advance of older observers, we are now able to discover tabes dorsalis before the stage of locomotor ataxy—in some cases, many years before—and I believe extended observations will do us similar service in the case of general paralysis, which has many interesting points of contact with tabes dorsalis. Such observations must, however, proceed in the main from the general practitioner of medicine, in whose training lesions of the nervous system have hitherto had sadly too small a place. The ability to detect the disease at an early period could not but throw some light on its pathology, and might give indications for the treatment of an affection which, because it is common and commonly fatal, deserves the closest attention of every student. Here I may be allowed to suggest a line of enquiry: What part does syphilis play in the production of general paralysis? Dowse (*Syphilis of the Brain*, p. 142) makes the following forcible statement: "I should say that syphilis, by the diffuse changes which it is known to instigate in the small vessels of the brain, is the cause of at least two-thirds of the general paralysis which leads to dementia, as we meet with it in this country." This, in my opinion, overstates the case. Of patients drawn from the agricultural class chiefly, I have seen but two, or perhaps three, whose affection could be attributed to syphilis, and it may be questioned whether in these cases the disease ought not to be called syphilis of the frontal lobe, which most writers hold should not be confounded with general paralysis. Without a history of syphilis, however, I believe attempts to distinguish between the two would often fail; and Dowse, commenting on a case of convolutional degeneration of the

anterior lobes following syphilis, italicizes the following statement: "This case is one of that series which comes under the title of general paralysis of the insane." (*Loc. cit.*, p. 136.) He proceeds to say that in seven out of twelve such cases under his care during four years, there was a distinct syphilitic history. The point is worthy of notice by those under whose care cases of general paralysis first come. As a rule, patients only find their way to an asylum when the disease is advanced; it is then difficult to obtain any previous history, and treatment avails nothing. I give brief notes of a case under observation at the Somerset Asylum; it is probable, looking at the previous history, symptoms, and *post-mortem* appearances, that the patient was suffering from syphilis.

W. H. B., aged 43, was transferred in September, 1882, from the Gloucester Asylum, where he had been sent as a wandering lunatic. He was in the navy for twenty years; recently he had been out of employment, and had undergone much mental anxiety. His habits were said to be sober. No further history was obtained, except from the patient himself, who stated he had had venereal disease badly; the state of his mind precluded him from entering into details. His condition on admission was as follows:

His expression is dull; during conversation he tends to be emotional; what he says is coherent, but he is unable to sustain conversation, his memory being very poor, especially for dates; he speaks slowly and with difficulty, the facial muscles trembling meantime; the tongue is protruded with a jerk to the right side, and is somewhat tremulous; the pupils are irregular in outline, equal, tending to dilatation; they react to light; his gait is weak; the sphincters are paralysed.

Three months afterwards the symptoms were more

serious: utterance is very thick and slow, and he slurs words; as far as it goes, his conversation is coherent. The paresis is very marked: he stoops, sitting and standing; in the latter position, he needs to support himself by his hands on a table, and he walks with great difficulty; he can preserve equilibrium with closed eyes; there is marked tremor of the hands. The paresis is more marked on the left side; on this side the knee-jerk is certainly exaggerated. There have been no convulsions or apoplectiform attacks—simply progressive paresis and dementia. He died a month afterwards.

Autopsy.—Body well nourished; no scars, no enlarged glands; calvarium dense, especially in front. On the inner surface of the left os frontis there seems to have been a new deposit of bone, much increasing the thickness of the calvarium in this region; cerebro-spinal fluid excessive. The brain weighs 47½ oz., and is very soft; the pia-arachnoid is very opaque over the anterior half of the convex aspect of both hemispheres, thick, and in some parts of milky whiteness. The right hemisphere is more involved; the pia mater is adherent to a great part of the first transverse frontal gyrus, to the central end of the third frontal and adjoining portion of the ascending frontal; the lips of the sylvian fissure are adherent anteriorly. The gyri generally are much wasted. The basal vessels are slightly thickened, and of bluish opacity. (The smaller vessels were, unfortunately, not examined for the changes described by Heubner.) The cord is firm and, to the unaided eye, healthy.

The heart weighs 10 oz.; the valves are competent; on section, the vessels in the left ventricular wall are seen surrounded by a large white halo, the appearance suggesting a new growth until the lumen of the vessel was found.

The capsule of the liver is thickened at isolated spots, and immediately under the surface at one point is a dark-grey, mottled nodule, not distinctly defined from the surrounding tissue, and of the size of a filbert.

We are now beginning to understand that the spinal cord plays an important *rôle* in general paralysis. Until recently it has been too commonly assumed that the cerebral hemispheres are the only or chief seat of the disease. Boyd and Bucknill long ago expressed their belief that the cord participated, but at that time microscopy spoke faintly, and it is comparatively lately that Westphal's observations have shown the correctness of their assertions. In this direction, most interesting relations are found to exist between general paralysis and other systematic lesions of the nervous system. Briefly, Westphal found: 1st. Affection of the posterior columns throughout their whole length. 2nd. Affection of the posterior portion of the lateral columns. 3rd. Affection of both posterior and lateral columns combined. I have myself found the above lesions in several cases; and I believe that were the cord more frequently examined, they would be found in a large proportion of cases. Hitherto they have received but little attention, notwithstanding their important bearings, clinically and pathologically. Here I would point out the value of a systematic examination of the deep reflexes in general paralysis, and, indeed, most other affections. The knee-tap is a sign requiring neither skill nor time to test, and its presence or absence, particularly the latter, will often throw bright light on a case. Dowse observes that he has often seen cases in the pre-ataxic stage of tabes dorsalis mistaken for nervous dyspepsia, nervous debility, exhaustion, hypochondriasis, &c., when a slight tap on

the patellar tendon would have prevented error. Conversely, I have seen a case diagnosed as locomotor ataxy which examination of the knee-jerk, followed up by closer examination of the symptoms, showed to be one of intracranial disease. The deep reflexes in general paralysis are altered more frequently than is generally supposed. One of my late colleagues, Dr. Beatley, has dealt very fully with this question in a graduation thesis, the publication of which I shall not anticipate further than by stating that in a large proportion of the cases he examined the deep reflexes were altered, and that, when so altered, the cases were divided almost equally into those whose patellar reflex was exaggerated and those in which it had disappeared. During the past three months I have admitted three cases of general paralysis into this asylum: in one, the knee-jerk is distinctly exaggerated; in a second, the knee-jerk is excessive, and accompanied by ankle-clonus as marked as I have ever seen it; in the third, the knee-jerk is absent. From former experience, I expect to find double lateral sclerosis in the second, and posterior sclerosis in the third case; in the absence of ankle-clonus, the exaggerated knee-jerk in the first case is not conclusive of lateral sclerosis. Now, sclerosis of the posterior part of the lateral columns is known to occur under the following circumstances: (1) interruption of the pyramidal tract by a focal lesion at any point before its entrance into the cord, the sclerosis being then most usually single; (2) a transverse lesion of the cord by injury or disease within or without the cord, the sclerosis being then probably double; (3) the disease known as *amyotrophic lateral sclerosis;* (4) that little-known disease, *primary lateral sclerosis.* Can the descending sclerosis of general paralysis be brought under any of these heads?

The condition of the brain and cord in one of my cases, in which there were increased knee-jerks and ankle-clonus, was briefly as follows: The pia-arachnoid was opaque and thick over the convex surface of the hemispheres, especially so over the frontal lobes and central gyri; there were no adhesions of pia mater to gyri, but the latter were much wasted; on section, the grey matter was found very thin; *no focal lesion could be found anywhere in the hemispheres, ganglia, pons, or medulla;* to the unaided eye there was distinct sclerosis of both lateral columns of the cord throughout their whole length. Such a case as this does not appear to come under any of the first three divisions, and we seem reduced to the conclusion that the convolutional lesion and the spinal sclerosis are independent events, explaining the frequent coincidence by saying that in general paralysis there is a tendency to the deposit of fibrous tissue throughout the nervous centres. A fatal objection to such explanation lies in the question, "Why should the lateral columns only display this tendency?" I suggest with diffidence another explanation: It is now believed that the pyramidal fibres are basal prolongations of certain giant cells in the third layer of the cortex in the Rolandic region of the hemispheres. Separation of the fibres from the influence of the cells, as happens when a hæmorrhage occurs in the internal capsule, leads to the change known as secondary degeneration. In general paralysis these cells suffer in common with other components of the cortex: "The larger cells further in, and the large multipolar cells, are more or less degenerated or atrophied, especially in patches and areas." (Clouston *on Mental Diseases*, p. 377.) Now, looking at the effect upon the issuing fibres of destruction or atrophy of the large cells of the anterior cornua of the cord, it

appears to me that a similar effect might be expected in the case of destruction of the cells originating the fibres of the pyramidal tract, and that the sclerosis in cases of general paralysis where no focal lesions can be found might arise in this manner. "It is not yet proved that destructive lesions limited to the grey cortical matter of the Rolandic region are productive of secondary degenerations. Nevertheless, some facts taken from the pathological anatomy of general paralysis tend to show that this is really the case." (Charcot *on Localisation of Cerebral and Spinal Disease*, New Syd. Soc., p. 196.) *

More difficult of explanation still is the connection between general paralysis and sclerosis of the posterior columns of the cord. Medical literature contains several reports of cases of tabes dorsalis terminating in general paralysis. Clouston (*loc. cit.*, p. 364) relates one and alludes to others; from the tabes point of view, however, the sequence seems to be regarded as rare. Looking from the other side, I believe it is a not infrequent event. Since my attention was directed to the point, I have, during the past two years, met with several cases of general paralysis in which the knee-jerk was absent; and in such as he had the opportunity of examining after death Dr. Beatley found well-marked posterior sclerosis. Invariably, loss of the knee-jerk was the most noteworthy

* Since writing the above, I find that Flechsig interprets the sclerosis as consecutive to cortical lesions. Recently, Westphal has combated this view, holding that it has not yet been demonstrated, and is of opinion that the sclerosis is primary; indeed, he believes that in general paralysis only has primary lateral sclerosis yet been described, for in Morgan and Dreschfeld's celebrated case the anatomical appearance pointed rather to amyotrophic lateral sclerosis than to primary lateral sclerosis. Lateral sclerosis occurs too frequently in general paralysis to allow one to regard it merely as a coincidence, and herein, I think, lies a serious objection to Westphal's view, which affords no explanation of the association between the lesions of the brain and the lateral columns of the cord.

sign of such lesion, other symptoms being apparently masked by the paralytic and mental state. There was nothing in the previous history (scanty as it generally was) of the cases pointing to early tabic symptoms. The only instructive history I have got was given by a patient now under my care. He is a painter and decorator by trade, and has a blue line at the edge of the gums. He denies ever having had colic, having been in the habit of taking a purge weekly. He has hard arteries, and marked arcus senilis. He denies syphilis, but says his wife miscarried four times. Seven years ago he began to suffer from symptoms which continued during three years: one was giddiness, which proved very inconvenient when he had to work on a scaffold. At one time he had dimness of vision, which suddenly disappeared; at intervals during this time he suffered much from *peculiar pains about the waist and in the legs, which he described by darting his finger rapidly forwards.* He is now unmistakeably suffering from general paralysis. I believe that in such cases as these the cord lesion is primary, though often undetected. As mentioned above, the pre-ataxic symptoms of tabes dorsalis are probably often misinterpreted, and to the disadvantage of the patient; for, if attacked early, the disease does not appear to be so obstinate as has been supposed. (Dowse, *loc. cit.;* Althaus, *Sclerosis of the Spinal Cord.*) In this connection, the importance of syphilis as a cause of tabes dorsalis will be at once observed. There is still a "missing link" between posterior sclerosis and convolutional degeneration. So far as I know, there is no detailed account published of the lesion of the cord being traced into the hemispheres. Clouston (*loc. cit.*, p. 364), indeed, says he has traced the spinal disease up through the medulla and the lower ganglia into the brain cortex; but

an *ex cathedrâ* statement cannot be fully accepted when we come to consider the complicated anatomical relations which exist between the posterior columns of the cord and the cortex—relations still ill-understood. It is as difficult to explain the association as to say why atrophy of the optic nerves, or paralysis of the oculo-motor nerves, should accompany tabes dorsalis.

An important pathological conclusion from the foregoing is, that two cases of general paralysis may differ *toto cælo* in their genesis; whilst in one case the morbid process may *commence* in the cortex, in the other it may *terminate* there. It does not lie within the scope of this paper to discuss the difference between the symptoms of these two varieties, and I shall not do more than mention that in many cases there is no well-defined cord-lesion found, agreeing with the absence of change in the deep reflexes during life.

I do not suppose that I have written anything which is not more or less familiar to those who have a large experience of general paralysis and a knowledge of its literature. My object has rather been such that this paper might have been entitled, "A Plea for a closer Investigation of the Causes and early Symptoms of General Paralysis by my brethren in general practice."

ON THE DIAGNOSIS OF ABSCESS OF THE LIVER.

BY

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ABSCESS of the Liver is not a disease of frequent occurrence in this country. It is principally to be found among residents in warm climates. Cases, however, are occasionally met with, especially among persons who have returned to England after spending some part of their lives in the East Indies or other tropical climate. Nor are those who have never left our own shores entirely exempt from attacks of hepatic abscess.

The remarks in the following paper will have reference principally to the diagnosis of liver abscess, and to some of the sources of error which may come in our way in attempting to form such diagnosis correctly.

The symptoms of this disease are often very obscure; they follow no regular course. In some instances they are unmistakeably prominent and clear; in others they are absent altogether. They often simulate diseases of adjacent organs, and frequently baffle the practitioner by assuming a mask which hides the real malady.

Rouis, in the cases collected by him in Algeria, found that the symptoms were only perfect in 8 per cent.; imperfect in 79 per cent.; and in 13 per cent. the disease ran a latent course.

Most writers of authority speak of the difficulty in certain cases of determining the presence of hepatic abscess from the almost entire absence of any symptoms to call attention to the liver. Frerichs, Budd, and Morehead record cases in which there were no hepatic symptoms, no pain or tension, diarrhœa or sickness, or any local discomfort—nothing but some amount of pyrexia; and yet such cases have proved fatal in a few days, and abscesses containing from half-a-pint to a pint of pus have been found in the liver.

Although it is not my intention to enter upon the pathological aspects of this disease, a few words are necessary as bearing upon the question of diagnosis, and especially with reference to the connexion between suppuration of the liver and dysenteric ulceration of the intestine. The almost exclusive pyæmic theory of liver abscess of some authors appears to me to be untenable; for example, 86 per cent. of the cases of hepatitis treated by Morehead recovered, and this is inconsistent with a theory of pyæmia. In Dr. Baly's cases of dysentery at the Milbank Penitentiary there was not one followed by hepatic abscess. The ulcerations of the intestine occurring in typhoid fever and phthisis are not often followed by hepatic abscess. In the Pathological Museum of the Army Medical Department at Netley there are 43 specimens of hepatic abscess; in 34 the abscess was uncomplicated with dysentery.

On the other hand, however, we have the authority of Annesley in support of the pyæmic theory. Of 29 cases collected by him, 21 had dysentery. A large proportion of Dr. Budd's cases also had a history of dysentery.

Without accepting an exclusive pyæmic theory, there is enough to show that a previous history of dysentery,

especially in the cases of persons who have resided in the tropics, should have its due weight in assisting us to form a correct diagnosis.

A previous history of dysentery has also this further significance, that it will enable us to calculate with some degree of probability as to whether the suspected abscess is single or multiple, as the latter form is in the majority of cases preceded by dysenteric ulceration, although it would be unwise to lay down any hard rule on the subject.

There is another form of inflammation of the liver of occasional occurrence among tropical invalids, I mean that involving the peritoneal covering, or peri-hepatitis. That this envelope should suffer, and very frequently *does* suffer, from exudative inflammation in connexion with abscess of the parenchyma of the gland, cannot be a matter of surprise; but inflammation of this structure alone is a rare disease, and when it does occur, seldom ends in suppuration.

The detection of an abscess in the liver is not always an easy matter. I have on several occasions seen physicians who have had long practice among such cases at fault.

The introduction of exploration by puncture and aspiration into practical medicine has greatly cleared away the difficulties surrounding this special diagnosis. It was formerly thought a terrible thing to plunge an exploring needle into such a gland as the liver, and, as it were, to prospect for pus. This is an operation constantly done now, not only without risk, but with absolute benefit to the patient, even though no abscess is reached. Hepatic pain and tenderness are frequently relieved by the abstraction of a small quantity of blood in this way, and in doubtful cases it is the best mode of solving the difficulty.

The following case is, I think, a good illustration of this doctrine: A soldier, aged 41, came under my care in 1873. He was a native of Lorraine, and had formerly belonged to the German Legion. When that corps was disbanded he entered the Line, and went to India, where he served fourteen years, and enjoyed fair health until November, 1872, when he was seized with dysentery. He recovered from this, and was subsequently attacked with hepatitis, but not in an acute form; in fact, from what I could learn from his official documents, the symptoms were more those of hepatic congestion than of any inflammatory condition. When he first came under my observation he complained of severe boring pain in the right shoulder. There was fulness and prominence of the right lower intercostal spaces; the whole hepatic region was enlarged and tender; tongue slightly furred; pulse about 100; temperature *normal;* no jaundice; no fluctuation could be detected; no pointing could be observed, nor any indication that the abscess (if one existed) was near the surface. There had been no rigors, no sweating (a symptom I have observed very frequently present in hepatic abscess), and the history of the case did not give much help. The question was, Was this a case of general enlargement and congestion of the liver, or was there an abscess? Exploration appeared to be the only means of discovering the truth. On the hypothesis that if it were an abscess, the collection of matter must be large, and it seemed hazardous to wait for some further change. An exploring puncture was therefore made in the eighth intercostal space in the right side. On applying the aspirator, only about 1½ ounce of bloody fluid was removed. A second puncture farther back was made with a similar result. If

there was an abscess, evidently it had not been reached. The local depletion, however, afforded considerable relief. After waiting for two days, another puncture was made a few inches still farther back. The abscess was now reached, and 12 ounces of fetid pus removed. Still, however, there was very little abatement of the pain, tenderness, and general discomfort of the patient, and no perceptible diminution of the hepatic enlargement. Accordingly a *fourth* exploration was made, and on this occasion with complete success, as I succeeded in removing no less than 96 ounces of pus by the aspirator. The fluid was measured in my presence, and the quantity was exactly six pints of 16 ounces each. I scarcely thought it possible that recovery could take place after such enormous destruction of tissue as must have occurred in this case, and yet this man recovered rapidly.

Various opinions were held among some of my colleagues as to the diagnosis in this case. Some doubted that it was an abscess of the substance of the liver, but held that it was a large collection of pus between the liver and ribs, resulting from peri-hepatitis, and circumscribed by powerful adhesions; and they based that view upon the conjecture that if the abscess had been situated in the liver itself, it must have totally destroyed the whole of the right lobe at all events, and that death would have been inevitable. Others suggested that the case was one of empyema mistaken for liver abscess, an error which has not unfrequently been made. The history of the case however, entirely excluded this view; while as regards the site of the abscess being external to the liver, resulting from suppurative inflammation of its covering, I can only say such cases are extremely rare. Morehead, whose experience in this

disease is vast, considers peri-hepatitis resulting in suppuration a very unusual occurrence. Then as regards the size of the abscess, and the apparent impossibility of any of the substance of the liver being left, we must bear in mind the extreme amount of distention which the liver will bear under these circumstances, and the rapid manner in which the cavities of abscesses not lined by very thick membrane contract after evacuation. In reference to the *size* of the abscess in this case, there are many cases on record of much larger collections of matter in the liver than this. In the Pathological Museum at Netley there are several specimens of larger abscesses—one especially, which contained *seventeen* pints. But the remarkable feature in this case was the rapid and satisfactory recovery after free evacuation.

Collections of purulent matter, however, do occasionally take place external to the liver, and between the opposing peritoneal surfaces, or between the liver and diaphragm, and give rise to symptoms which are with great difficulty distinguished from those of hepatic abscess. One case is fresh in my memory of this kind, and in which the diagnosis formed at first was entirely wrong. The patient had served for some years in India, and had experienced the usual diseases to which men are generally exposed there, occasional attacks of dysentery and "liver," as it is called; but no definite symptoms of abscess until his arrival in England, when he came under my notice. There was great bulging of the right side, and distinct fluctuation between the eighth and ninth intercostal spaces in front. A puncture was made, and several ounces of pus discharged. The opening was kept free, and the discharge encouraged. Eventually several other openings formed spontaneously, and the case was

treated as a liver abscess by drainage. After a long illness this patient died. There was no abscess of the liver at all; that organ was perfectly healthy; but between its external surface and the ribs there was a circumscribed abscess containing six to eight ounces of pus, and there was extensive ulceration of the structures on the inner surface of the ribs, with which the external fistulous openings communicated. From this case and some others I have met with I have been led to the opinion that when an abscess forms in this locality, between the external surface of the liver and the ribs, it generally makes its way to the surface by *several* openings; while if the abscess be seated within the liver, if it points externally, it is generally in one direction, and opens by a single orifice, the point of direction being most frequently, so far as I have observed, close to the free margin of the right lower ribs.

Abscess of the liver sometimes presents symptoms which may be mistaken for those of ulcer of the stomach, as the following case exemplifies: The patient had been a resident in India. There was no history of dysentery or (so far as we could ascertain) of liver disease. He was in a very weak, emaciated, and anæmic state. There was one spot of tenderness in the epigastrium; pressure upon this excited nausea. There was frequent and incontrollable vomiting and pain after taking food. There was no discomfort in the side, no bulging, no increase of the area of hepatic dulness, no elevation of body temperature during any part of the day. The patient died eventually, and an abscess was found in the upper and posterior part of the right lobe of the liver.

In another case of which I have the notes, the patient had returned from India in a very anæmic state. There was incontrollable vomiting. One tender spot was found

on the epigastrium not larger than a half-crown piece; but manipulation of the hepatic region generally was borne without shrinking. In this case there was no history of dysentery. The diagnosis was doubtful at first. High temperature and night sweats, however, supervened, and became constant, and this cleared up the doubt. The case proved fatal. There were several abscesses in the liver; one especially was found bulging from the posterior surface, involving the œsophagus and cardiac end of the stomach. These parts were matted together in a mass.

These two cases illustrate the significance of persistent and incontrollable vomiting as a symptom of hepatic abscess. I will refer to this point farther on.

Cancer of the liver, that is, the medullary form of the disease, sometimes leads to the suspicion of abscess. I attended a physician in practice near Southampton, in consultation with Dr. Orsborn, of that town. In this gentleman's case there were some doubtful signs. There was a soft globular fluctuating tumor situated low down in the hepatic region. It was not very painful or tender. We gave the patient the benefit of the doubt, and explored the tumor. A very small quantity of turbid fluid was withdrawn. An examination of this, and the fact that there was a distinct sulcus between the upper edge of the tumor and the free margin of the false ribs, led us to the conclusion that the case was one of malignant growth from the lower edge of the liver, which view was proved to be correct at the *post-mortem* examination.

Distention of the gall-bladder and echinoccoci passing into suppuration, may each give rise to the suspicion of hepatic abscess; but the diagnosis in such cases is easy in comparison to those cases where there exists pleuro-pneumonia at the base of the right lung, accompanying

suppurative inflammation of the liver. In such cases there is, however, a degree of prostration which no amount of stimulating food seems to alleviate, and which leads to the suspicion that there is a graver complication than merely inflammation of the lower lobe of the right lung and pleura; and, of course, the previous history either of dysentery or tropical residence will aid the diagnosis.

Abscess of the lung may be mistaken for hepatic abscess, as shown in the following case: A soldier, aged 38, was attacked with severe hepatic pain. The organ was apparently much enlarged. He had cough, but no expectoration. Shortly after admission he was seized with exacerbation of pain apparently in the liver, and expectorated a large quantity of pus, streaked with blood and very offensive. The case appeared to be one of hepatic abscess bursting through the right lung, and was treated as such. Gradually the expectoration declined and the health improved, when a second time there was a sudden aggravation of the above symptoms, with signs of breaking up in the lower lobe of right lung. The patient eventually died. The right lung was found compressed in its upper part; a large abscess was found in the lower and middle lobes, the lower boundary in intimate connexion with the diaphragm and convex surface of the liver, the former being very thin and reduced to the consistence of tissue paper. There was no abscess in the liver, or any trace of former inflammation in that organ.

The following case is an illustration of an almost directly opposite condition of things; namely, the existence of an hepatic abscess bursting through the right lung, and afterwards mistaken for an advanced stage of phthisis. A man, aged 25, was invalided from India for fever. On the passage home he had several attacks of

ague (so called, but doubtless they were rigors indicating the formation of abscess); he also had suffered from acute dysentery. On his arrival in England he was found to he suffering from cough, purulent expectoration, and night sweats. The physical examination indicated a disorganised condition of the right lung from top to bottom. The case was regarded as one of rapid phthisis. The expectoration was sanguino-purulent at first, afterwards purulent. Death took place from exhaustion and hectic. At the *post-mortem* examination the left lung was found healthy; the lower and middle lobes of the right were found infiltrated and broken up by purulent deposits, the lower lobe being attached to the diaphragm, through which an enormous abscess of the liver, containing nearly three pints, had burst.

It may be said that the history of the case, the condition of the right apex, and the character of the sputa should have directed the diagnosis; but when cases of this kind are seen, as they most frequently are in this country, only in an advanced stage, the diagnosis is difficult. The dark prune-juice expectoration, characteristic of liver-abscess, has often ceased, and it has become simply purulent. Microscopic examination of the sputa may help us, but it is not always to be relied on.

The symptoms of hepatic abscess may be complicated with others of so formidable a character as to throw doubt and uncertainty over the whole diagnosis. The following case is an evidence of this fact; the case presents some unusual clinical and pathological features: J. H., aged 29, a man of temperate and regular habits; served for five years in India. During his service there he suffered from ague on three occasions in 1871-72. He never had hepatic disease or dysentery in India.

From 1873 until 1875 he continued in good health, when he received a gunshot wound of the hand, which necessitated his being invalided, and he arrived at Netley on the 2nd February, 1876. The wound was healed. He was admitted to the medical wards for apparently a feverish cold. On the 4th he was pale; his breathing and pulse both accelerated. After careful examination a small prominence was noticed over the left lobe of the liver, about two inches below the sternum, and in the mesial line. It was rather hard on pressure, but neither tender or painful. Pressure caused a slight feeling of nausea. There were no rigors or perspirations. Next day the tumor had increased in size, and on that evening the temperature rose to 104, fell next morning to 100, and fluctuated between that point and 102 until the 7th, when it suddenly fell to 97, above which point it never rose again. The tumor increased in size, became more defined, with a boggy sensation, and with a distinct sense of *pulsation* at the sides. The patient had several attacks of syncope. On the 11th, at 8.45, he passed about 1½ pint of blood by the rectum. The temperature sank to 96·4. There continued to be occasional vomiting, faintings, and very rapid pulse until the 13th, when the temperature sank to 95. He died on the 14th, twelve days after his admission to Netley Hospital.

At the *post-mortem* examination the following conditions were found: There was an abscess, as at first supposed, in the left lobe of the liver, its walls without any distinct lining membrane, its contents very thick and purulent. The liver was pushed forward by the transverse colon, which projected below it; and this, together with the whole of the large intestine, was distended with blood and quite black. The small intestine was through-

out normal except in calibre, being in its lower part reduced to the diameter of a lead pencil. In the caput coecum, but not elsewhere, there were *three* ulcers, each of the size of a sixpence, with deep bases and prominent ragged surfaces. Evidently the hæmorrhage had taken place from these, and had gradually filled and distended the large intestine with partially coagulated blood. Its passage from the anus had been partly prevented by a constriction near the orifice, the remains of old ulceration.

This case appears to me to present many points of interest; but space will not permit me to do more than to remark that the diagnosis was complicated and obscured by the insidious onset and subsequent rapid progress of the symptoms, by the position of the tumor, by its pulsation, and by the unusual appearance of hæmorrhage, syncope, and depression of temperature.

Collections of fluid in the right pleura may be, and frequently are, mistaken for abscess of the liver. I have seen several instances of this kind during the last ten years; in one case notably, where the pleuritic effusion had formed a pouch behind the diaphragm, and had pushed the liver forward very prominently. It is always best with obscure signs of this kind to make an exploratory puncture.

What, then, are the signs and symptoms most to be relied on in forming a diagnosis of hepatic abscess? In answering this question I would exclude those cases of acute hepatitis resulting in abscess, rarely seen out of the tropics. In such cases the diagnosis is comparatively easy. The pain, high temperature, rapid pulse, hepatic tenderness, frequent vomiting, and successive rigors, all point to the formation of abscess. This condition of things is rarely met with in Indian residents returned to England. The acute symptoms are seldom observed. If

they have existed they have passed away. The abscess, if there be one, may have already formed, and all symptoms of inflammatory action may have ceased.

In those cases which present a great similarity to fever of an intermittent type, careful observations of temperature may be of material assistance.

When hepatic abscess is accompanied by frequent rigors the temperature rarely falls to the normal standard during the intermission, as it does in ague. The fever paroxysms are also of shorter duration, are more irregular in their rhythm, and are seldom influenced by the administration of quinine.

Hepatic pain, although present in the majority, at least, of acute cases, is not a constant symptom in those of a more chronic character, nor is it very severe.* The liver is not a very sensitive organ; and, unless the coverings of the organ be involved, the pain experienced by the patient is seldom commensurate with the amount of local mischief. Tenderness on pressure is, however, a sign which I have hardly ever observed to be absent in hepatic abscess; but the amount of tenderness depends mainly upon the position in which the abscess is situated. The cases in which tenderness is most marked, are those where the abscess is situated on the concave surface of the gland.

Increase of the area of hepatic dulness is an important sign in diagnosis of liver abscess, but we must look for it in the upper margin. The lower margin generally remains within its normal limits, unless the abscess be of very large size. Auscultation, I need not say, is of great importance in enabling us to determine the relation between the lung and upper margin of the liver.

* The sensation is one more of discomfort and fulness rather than fixed pain.

Enlargement of the liver, by which I mean general swelling of the organ in all directions, is not of frequent occurrence in suppurative inflammation. This fact has been specially noted by Morehead, who explains it upon the hypothesis that the nutrient vessels, namely, the capillaries of the hepatic artery, are those most engaged in inflammation; that their capacity is much smaller than those of the portal vein; and that this in a great measure accounts for the fact that the bulk of the organ is little increased in inflammation compared with congestion, a deranged condition in which the large portal capillaries are directly implicated. And certainly this view, if correct, tends to explain another fact, that the function of secretion is seldom interfered with in cases either of acute or chronic suppurative inflammation of the liver. I do not remember ever to have seen jaundice occurring in the course of an hepatic abscess. Out of 318 cases Morehead only observed jaundice in *five;* its presence or absence is, therefore, of no account in determining the diagnosis. There is, however, a dusky condition of the skin, especially of the face, neck, and upper part of the trunk, almost constantly present in these cases. Whether it be the result of the circulation of bile in the tissues, or from pigmentation following long exposure to malarial influences, I am not prepared to say.

Incontrollable vomiting occurring in a patient with a tropical history is a symptom possessing considerable diagnostic value. This has been pointed out forcibly by my late colleague in the Army Medical School, Professor Maclean, in an article published in the *British Medical Journal* in August, 1874. Dr. Maclean there cites several cases showing the significance of this symptom.

Sir Joseph Fayrer, the present physician to the Council

of India, in commenting on Dr. Maclean's paper, draws attention to the insidious manner in which the liver passes into a condition of suppuration, not only in the cases of persons residing in India, but also in those who have left the country, and refers to the point, which has been borne out by his experience, that vomiting, as a symptom of liver-abscess, will depend much upon the position and size of the abscess and upon the pressure it may exercise on adjacent organs, or to the amount of irritation it may excite in the vagus nerve. The two cases I have already cited bear out this observation.

There are some cases of hepatic abscess of insidious origin, however, where there are no symptoms, and where the first indication that an abscess has existed may be the evacuation of a large quantity of purulent matter by the bowels.

I would say, in conclusion, that if in the cases of persons formerly resident in India who have returned to this country, either with or without any history of previous hepatic or intestinal derangement, we should find symptoms of malaise, more or less pyrexia, evening chills followed by perspiration not controllable by quinine, a malarial aspect, dingy conjunctivæ, vomiting difficult to subdue, with irregularity of the bowels, we may have grave suspicions of hepatic abscess; and if, in addition to these, there should be pain and discomfort in the region of the liver, sympathetic pain in the right shoulder, cough, dyspnœa, tenderness on pressure, tension of the right rectus muscle, and any prominence of the right side, our suspicion may be reduced almost to certainty. In such circumstances an exploratory puncture removes all doubt.

Clinical Records.

A CASE OF LARYNGEAL PHTHISIS, IN WHICH THE WINDPIPE WAS OPENED ON ACCOUNT OF DYSPNŒA.

By CHRISTOPHER ELLIOTT, M.D., Physician to the Bristol Hospital for Sick Children.

E. R., æt. 6, the child of a dock labourer, was admitted into the Children's Hospital, under my care, with the following history: For the last six months she had been gradually losing her voice, and her breathing had become bad. She had no cough in the daytime, but at night would get attacks of dyspnœa and coughing. She never had suffered from croup, but three years ago had a sharp attack of broncho-pneumonia. There was no history of syphilis; both her parents and their other children were all healthy and living, but a maternal aunt had died of consumption. The day after admission it was noticed that the patient had some difficulty in swallowing food, especially liquids. She had one or two enlarged glands in the upper part of the neck on both sides, but there was no swelling in the pharynx, and no tenderness or deformity over the upper cervical vertebræ. As I was away for a holiday when my patient was admitted into the hospital, I did not see her until September 7th. Her respiration was then noisy and stridulous. She had no cough, and could not speak above a whisper. The head was thrown

backwards, and kept fixed in this position ; the sterno-mastoid muscles on both sides were tensely contracted, and the lower end of the sternum was drawn in deeply at each inspiration. Over the third rib, a little below the right clavicle, was a hard osseous nodule. Physical examination of the chest showed that there was very little air entering the lungs, especially the apices, and some slight dulness in front over the left apex. All attempts to ascertain the condition of the larynx with the laryngoscope failed, in consequence of the dyspnœa and spasm caused by the introduction of the instrument, and the collections of mucus and saliva at the back of the throat. In the night of September 12th the patient had an unusually severe attack of dyspnœa, and the next morning, after a consultation with the other members of the staff, my colleague, Mr. Norton, performed laryngo-tracheotomy. The operation was skilfully done, and gave immediate relief. The stridor and dyspnœa at once disappeared, and afterwards the patient was able to take food much better, only a little fluid now and then coming through the tube. For a fortnight the patient seemed to improve and pick up a little bit; but after this she began rapidly to waste and lose strength. The dulness over the left lung increased, and the right lung also became dull. The evening temperature rose to above 103°, night sweats set in, and the patient died on October 14th. Five days before death took place it was noticed that respiration was carried on partly through the nose and mouth, and partly through the tube ; and when the tube was completely blocked, the patient could breathe without distress. The tube was therefore removed, and the opening allowed to close ; but the voice could not be raised above a low whisper.

Post Mortem.—Six hours after death, the tongue, larynx, œsophagus, lungs and heart were all removed together for examination.

Larynx.—No trace of vocal cords, either true or false. The whole of the laryngeal mucous membrane was pale and thickened, and had patches of ulceration. The epiglottis was a little thickened, but not ulcerated. On the right side of the larynx, near the posterior part and above the position of the right vocal cord, a small abscess cavity was found, which had burst, and was probably the size of a large pea when full, and had been most likely the cause of the attacks of suffocation the patient had suffered from. Below the thyroid cartilage, the trachea seemed healthy, and there was no sign of irritation where the tube had been worn.

Œsophagus was quite healthy.

Lungs.—The left lung was moderately adherent to the chest wall over most of its surface. It was full of miliary tubercles, about the size of a pin's head, and scarcely crepitant in any part. Right lung: the upper lobe was solid, and full of same tubercles. There were no cavities in either lung.

The bronchial glands were all considerably enlarged, but not cheesy. The *heart* was normal. The liver and kidneys were free from tubercle. Both *recurrent laryngeal nerves* were dissected out in their entire course, and found to be embedded throughout a large part of their course in enlarged glands. The nodule felt during life over the third rib turned out to be an old fracture of this rib, which had badly united, and was joined on to the one above it.

Remarks.—The necessity of opening the trachea in laryngeal phthisis very rarely occurs, according to Morell

Mackenzie. In his book on *Diseases of the Throat and Nose*, he gives a list of 500 cases, and in three only was there such extreme dyspnœa as required to be relieved by tracheotomy. Though the operation prolonged my patient's life only for a month, yet it enabled her to pass her last days in comparative ease and quietness, without being liable to distressing suffocative attacks at night, or whenever she partook of food. It is remarkable that, notwithstanding the complete destruction and disappearance of the vocal cords, this patient had no difficulty in making known her wants, as she was always able to speak in low whisper; and I was not prepared to find such a condition of parts in the larynx as the autopsy revealed. The finding of the abscess cavity in the posterior part of the larynx, above the site of the right vocal cord, accounts, I think, for the dyspnœa she suffered from, and its bursting for the disappearance of this symptom before death.

NOTE ON THE AFTER TREATMENT OF SCARLET FEVER BY SCALDED OATMEAL.

By GEORGE SMITH, M.R.C.S., Axbridge, Somerset.

As the heading of this Note implies, it is intended here to treat of the subject of the desquamation which follows every case of scarlet fever, however slight, both in regard to its bearings on the patient himself and also those with whom any cast-off particles may come into contact.

Take first the process of desquamation. This, as we all know, varies very much in different individuals, and sometimes it is done by particles so fine as to be hardly perceptible; and these are, I think, a very frequent and most certain source of contagion, by means of clothes and otherwise—much more so indeed than the scales as

ordinarily thrown off; and I may here state that it is within my own knowledge that the contagion has been thus carried from one house to another, more than a hundred miles apart, at the end of at least a year from the attack.

Now, to obviate this danger, I have for several years been in the habit of having my patients sponged over the whole surface of their bodies twice a day—commencing, as a rule, about a week from the appearance of the eruption, and continuing the process until desquamation is complete—with a mixture of one ounce of oatmeal to one pint of boiling water; the solution to be made fresh every day and used tepid, or at such a temperature as may be comfortably borne by the back of a finger.

My reason for using this particular combination is, that the gluten in it sticks the scales to each other and to the surface of the body, thus allowing of their being removed, from one sponging to another, without the ordinary risk of infecting either atmosphere or clothes, and greatly lessening the risk of spreading the disease.

Secondly, this same gluten fills up the cracks of the new skin and protects it from cold as, patch after patch, it becomes bare, and thus, to say the least, greatly lessens the risk of the dropsy which so often follows upon this disease.

TWO CASES OF HÆMOPHILIA. By the EDITOR.

These two cases, which have been under my observation during the past year, present, in conjunction, a very complete clinical study of the Hæmorrhagic Diathesis, or Hæmophilia.

Case 1.—The first patient, an exceedingly delicate lad of 17, was admitted to the Bristol Infirmary in February,

1884, with bleeding from a scratch on the ball of the thumb, which had been going on for three weeks. All the orthodox plans of treatment appeared to have been tried, and the result was a sloughy sore, as big as a florin, from which the blood welled up freely on the removal of pressure. I stopped all pressure, put him to bed, elevated the arm, and applied a simple dressing of compound tincture of benzoin. The bleeding diminished considerably, but still continued after three days. I then applied Ruspini's styptic locally, and in two days it had quite stopped, and the wound rapidly granulated up. Large doses of the perchloride of iron and of gallic acid were given internally.

After a fortnight he appeared to be in his usual state of health, when, after a restless night, he awoke with his right elbow-joint very painful and full of fluid. In three days this fluid disappeared, and the joint recovered its ordinary mobility.

The blood which came from the wound was of the ordinary watery nature after prolonged hæmorrhage. Tested by Gomers's Hæmactyometer, the hæmoglobin was found to be $\frac{6}{10}$ of the normal, and the corpuscles 1,600,000, instead of 5,000,000.

This boy had had a large and varied experience of hospitals and surgeons. When fifteen months old he was in St. Thomas's Hospital for a swelling of the knee, which appears to have been called white swelling. When eight years old he was in the Bristol Children's Hospital with a swelling in the same joint, to which the same name was given. When eleven he was in the Bristol Infirmary, under the late Mr. Tibbits, with fluid in several of his joints. The condition was then diagnosed as one of hæmophilia, and one of his joints was aspirated, pure

blood being removed, but almost immediately refilled. So peculiar was his appearance, with wasted limbs and swollen, distorted joints, that he was photographed. Since then he has been frequently a patient at the Infirmary with bleedings of various sorts, spontaneous from the nose and lips, or traumatic from scratches or cuts.

The most prominent clinical feature of the case is the joint-swelling. Every large joint in his body has been, at one time or another, affected; and on every one of the frequent occasions when I have seen him, at least two of his joints have been distended with a fluid, which is, doubtless, blood. Usually the joint fills spontaneously in a few hours, causing him much pain, and subsides in a few days. Sometimes a twist or wrench is the cause of the hæmarthros. Even rough handling may be a cause. For instance, he was exhibited at a local medical meeting, and his wrist was several times grasped to move his elbow, which was full of fluid. Next day his wrist-joint also was full of fluid, and the tissues around were ecchymosed. With him a sharp tap will cause an ecchymosis.

Occasionally he appears at the Infirmary with a "taste of blood in his mouth," from oozing through the lips or gums. Blood has not been observed in urine or fæces, and it is now some time since he has had bleeding from the nose.

The heredity of the complaint is, as usual, on the mother's side, though, in spite of careful inquiries, no more than one generation can be accounted for. One brother died in childhood, of bleeding from the opening by incision of a small abscess of the scalp. Four sisters and three brothers have grown up strong and well without showing any signs of the diathesis. His mother had a

brother who died of bleeding. These are all the facts bearing on history which the meagre knowledge of the family about their relatives can supply.

Case 2.—This was a child fifteen months old, who was admitted in July, 1884, for bleeding from superficial abrasions on his finger and palm, caused by a fall. This happened twelve days before admission. He had had medical advice, and all the ordinary remedies were tried. He was absolutely blanched, looking more like a wax model than a living child, and was very restless, and had begun to vomit. The sores had begun to slough from the pressure exerted.

All pressure was removed, the arm was fixed on a splint, and simple turpentine dressing applied. The bleeding at once stopped, and the wounds soon healed up. At the end of three weeks he was discharged, having gained strength with rapidity.

This was the first attack of bleeding which the child had had; the mother believed it was the first wound he had received. The child had not shown signs of constitutional weakness, but had always been liable to get extensive bruises from trivial causes. In fact, he was never without one or more areas of ecchymosis. There had been no joint-swelling or bleeding from any of the mucous membranes.

The diathesis here also is on the mother's side. Two maternal uncles died of hæmorrhage; one in childhood, from biting his tongue; the other at twenty-four, after the extraction of a tooth. Farther back than this the family records cannot be traced. He has one brother, aged twelve, who is a bleeder and also suffers from hæmorrhages under the skin, and from the mucous membranes of the nose and mouth. No other relatives have shown signs of bleeding.

I have seen three other cases of hæmorrhagic diathesis. One was in consultation with Dr. Lionel A. Weatherly, of Portishead, where there was severe epistaxis. This patient had had blood in two at least of his joints, and bled from the kidneys as well. Bleeding from ordinary cuts and scratches were usually stopped without much difficulty. The two others were under the care of colleagues at the Infirmary, and presented no new features.

Remarks.—It is needless that I, who have nothing new to say, should add to the already voluminous literature of the subject. Perhaps Dr. Wickham Legg, who has made the subject peculiarly his own, may, in the forthcoming second edition of his work on *Hæmophilia*, help to throw some light on its pathology. So far as the modes of investigation as yet applied can help us, its pathogeny would seem to be neither in the structure of the blood-vessels nor in the composition of the blood. Would it be worth while to examine the nervous system as well? Might it be that the nervous force which causes the arterial muscles to contract is deficient or wanting? Is it a paralysis of the arterial muscles?

As to treatment, I believe that the best plan, if we had the courage to adopt it, would be to throw physic to the dogs, and let the patient alone. Absolutely perfect rest in bed, mentally and bodily, locally and generally, with milk diet, and the simplest of dressings lightly applied, I believe to be all that is required. The actual cautery (*pace* Holmes) is to be unhesitatingly condemned. It makes matters tenfold worse, by substituting a slough with bleeding from big and deep vessels for an abrasion with mere oozing from superficial vessels. Strong pressure, powerful astringents, and everything which tends to destroy weakened tissues, are to be avoided. They tide

the patient over the first danger to leave him in a second and greater one. If we must plug the nares to save the patient's life, let the pressure of the plug be of the lightest, and let it be removed at least every twenty-four hours. As a hospital resident, I was actively concerned in the death of a patient through repeated plugging of the nares, and consequent ulceration of the mucous membrane. In a case of true hæmophilia, nothing less than imminent death would make me plug the nares. Lint soaked in turpentine I believe to be the best local application.

Constitutionally, I have administered gallic acid in fifteen-grain doses every three hours, turpentine in drachm doses every two hours, purgative salts and Ruspini's styptic—all during the period of bleeding. In the intervals, perchloride of iron has the best reputation. But, with the exception of the iron, I doubt if equally good results might not be got without any of them. Opium, I think, ought to be always administered during the bleeding. It quiets the restlessness, and so provides at least physical rest.

In conclusion, and specially with reference to the manner in which the disease is inherited, through the maternal grandfather—a peculiarity which it shares with colour blindness—I venture to suggest that we should note any peculiarities in the transmission of other dyscrasiæ or malformations. On working up the literature of such well-known subjects as cleft palate and extroversion of the bladder, I find a remarkable deficiency in the accounts of modes of inheritance; and a more superficial investigation in other directions shows a similar lack of information.

LATERAL SCLEROSIS OF THE SPINAL CORD.

By J. E. SHAW, M.B., Physician to the Bristol Royal Infirmary.

A. B., æt. 13, admitted to the Bristol Infirmary, Feb. 7th, 1884.

History.—Patient is the fifth of nine children, of whom seven are still alive. Her mother states that when born the patient was a very large child, and that the labour at her birth was painful and tedious, lasting three days; the midwife in attendance then sent for a doctor, but before his arrival the child was born, so that no instrumental interference was resorted to. No special weakness or rigidity was noticed during infancy; but at two years of age, as she could not walk like other children, she was taken to Dr. Budd, of Worcester, who gave it as his opinion that it was due to laziness, and advised that the child should be encouraged to crawl about. When four years old she was taken to the Worcester Infirmary, attending there as an out-patient under Dr. English for two years, off and on; she was quite unable to walk, and was carried every day to and from school. At seven years of age she could read a little, and was not markedly behind other children in mental power. About six years ago her mother took her to the Birmingham Hospital for Children, where she was examined by Dr. Carter; admission as an in-patient was refused, but the mother was warned against letting the child be frightened or fall into the fire, and so forth. Shortly after this crutches were made for her, and she succeeded, for the first time, in walking without the assistance of another person. At that time she walked upon her toes, the heels being raised

from the ground, as they are now. She has made no improvement in walking since then, but has progressed in intellectual development.

She has never had any kind of fit or convulsion. Has always been awkward in the use of her hands, especially of her right hand, and is therefore preponderatingly left-handed.

State on Admission.—Patient is somewhat below the average stature of her age, but not below the average of bulk and general development. Her eyes are staring, and her face expressionless, whence her appearance is rather that of an imbecile; this weakness of mind is, however, more apparent than real, as she is intelligent when led into conversation, and is a good-tempered and docile child.

When lying quietly in bed her legs are in a state of rigid extension, and strongly adducted, so that they cross one another if not kept forcibly apart. In sitting they can only be flexed to an obtuse angle, so that she sits with her legs stretched out, with the feet extended and the toes inverted. On being helped on to her feet, spasm of the muscles of the calf at once raises her on to her toes, the heels being elevated quite four inches from the ground. If she remains quiet this spasm gradually subsides, and the heels come lightly to the ground in something less than a minute. In attempting to walk, this upward jerk occurs at each step, causing a curious hopping gait; but at each step only one act of elevation occurs, not several alternating elevations and depressions. In moving forward, the toes cannot be completely raised from off the floor, but are scraped along it, and the spasm of the adductors of the thigh compels the passive leg and foot to be circumducted, as it were, round the other one

by means of the abductors of the thigh, in order that it may be carried beyond the active one, across the front of which it is finally drawn. Patient cannot walk without some assistance; as she stoops forward so much, the support either of another person or of crutches is necessary to prevent her falling prone. No cutaneous plantar reflex is obtainable, and no contraction is produced by irritating the skin on the inner side of the thigh. No ankle clonus obtainable, apparently because the degree of tension in the tendo achillis is too great to permit of its occurrence. Patellar tendon reflex is exaggerated greatly beyond normal: the slightest tap upon the tendon, the body of the quadriceps extensor near the patella, or upon the upper part of the tibia, produces with curious suddenness one strong contraction of the quadriceps extensor, followed by two or three smaller ones; and a rapid succession of taps upon the patellar tendon keeps the limb extended in tonic rigidity for a short time. These phenomena are more marked in the right leg than in the left, which is rather less rigid than the right lower extremity. The muscles of the lower limbs are well developed, and show no sign of atrophy. Faradic current increases the rigidity without producing any distinct movement of the limbs; 10 cells of a constant-current battery give CCC very powerful, COC weak, ACC medium, and AOC strong. Sensation is quite unimpaired. Her hands and fore-arms are affected in a somewhat similar manner to her inferior extremities, though to a much less degree. In the hands and fingers there is little rigidity, but much loss of power. The fingers are capable of independent flexion and movement in the left hand, and to some degree so in the right hand also, but they are incapable of executing any performance more delicate than knitting, any attempt at

sewing being a ludicrous blunder. The tendon and periosteal reflexes associated with the biceps, triceps, flexors and extensors of the fore-arms, and interossei are all greater than normal. Sensibility of the skin of fingers does not seem to be diminished, nor is there any marked tendency of the fingers to be clenched upon the palms, nor to the production of the *main en griffe*. The trunk of the body appears normal. There is no deep furrow separating the thorax and abdomen, and the abdominal skin reflexes are fairly well marked. The functions of the rectum and bladder are normal, and sexually she is developed somewhat precociously.

The speech is rather indistinct, from imperfect articulation, but there is no aphasia proper.

The girl remained under observation for some little time, but did not vary at all in her condition, and indeed does not seem to have done so of recent years, being in the stage in which most cases remain for a long time.

This case, so completely typical of its kind, I have described under the title of Lateral Sclerosis, because we should expect to find sclerosis of the pyramidal tracts, if not of the whole lateral columns, as the anatomical basis of the lesion, and because the disease is most generally known by that name. On the other hand, I have not ventured to call it a case of Primary Lateral Sclerosis, because the circumstances of the case tend rather to make one believe the sclerosis to be secondary, and because, also, primary lateral sclerosis of the spinal cord seems to remain still a sort of pathological *ignis fatuus*, especially now that Dr. Dreschfeld's case has proved, upon farther investigation, to have been complicated with changes in the ganglion cells of the anterior cornua.

The history of a lingering, difficult parturition process,

the absence of motor power in the lower limbs during infancy, and the development of rigidity in them during the earlier years of childhood, lead one to the belief that this condition depends primarily upon a lesion of the excito-motor centres on each side of the brain (though not to the same degree on each side), caused by pressure of the parietal bones upon the subjacent convolutions at the time of labour. These psycho-motor centres having been more or less destroyed in the course of months, secondary degeneration spreads from them downwards through the internal capsule into the pyramids, and thence into the pyramidal tracts in the cord; or, perhaps more truly, these cerebral centres and pyramidal tracts in the cord do not proceed to complete normal evolution, as growth proceeds, after injury at birth; a condition to which Heschl has given the name of Porencephaly.

The case, therefore, is a variety of bi-hemiplegia, with late rigidity, if this theory be adopted as the correct one. The only point which seems to tell against the theory of bi-lateral hemiplegia is that the inferior extremities are much more severely affected than the superior. Dr. Gowers suggests to me that the overlapping of the edges of the sagittal suture causes more pressure upon the leg-centres, which lie nearer to the longitudinal fissure, than upon the arm-centres, which lie lower down.

Until a case of this description has been the subject of careful *post mortem* examination, some obscurity will continue to rest upon the precise extent and distribution of the lesion.

Notes on Books.

The Elements of Physiological Physics; an outline of the Elementary Facts, Principles, and Methods of Physics; and their applications in Physiology. By J. M'GREGOR-ROBERTSON, M.A., M.B., C.M. London: CASSELL AND COMPANY. 1884.

This volume, the latest of the "Manuals for Students of Medicine" published by Messrs. Cassell and Company, forms a companion volume to others of the same series. The first of these, Klein's *Elements of Histology*, has already become well known amongst students, and has achieved a considerable popularity amongst the class for whom it was intended. The second of them, Power's *Elements of Human Physiology*, gives in less than four hundred pages a general outline of the physiology of man, the details of structure being passed over in silence, and all descriptions of instruments and methods of procedure being omitted. The third, on *Physiological Physics*, comprises 528 pages, and has 219 wood engravings. This volume supplements the omissions of the others, giving descriptions of instruments and details as to their use. The author has also given the most important elementary facts and principles of Physics as a preliminary to the more detailed descriptions of the physical apparatus and methods adapted to physiological purposes. To the average medical student, to those who aspire to the higher degrees, and more especially to those who intend to become familiar with the modern methods of investigation of physiological and pathological problems, this book will be an invaluable companion. It will to some extent compensate for the want of familiarity with the subject of Experimental Physics as it is now taught in many of our "University Colleges and Technical Schools." A course of Modern Physiology should, if the time of the student permitted, commence with such a course of Physics and Chemistry as will enable him to understand, and utilise for purposes of research, the various physical and chemical methods on which the modern development of Physiology

has depended. The want of this knowledge is a constant source of annoyance both to the student and the teacher, and accordingly both the study and the teaching of Physiology have become increasingly difficult because of the broadening of the basis on which the science is now built. It has been frequently remarked that a mere sight of the pages of a modern journal of Physiology is sufficient for the average practitioner, who will scarcely pretend to understand, or attempt seriously to read, pages which would be almost as intelligible if written in an unknown language.

In this book the various branches of Physics are dealt with in order—electricity, magnetism, hydrostatics, hydrodynamics, pneumatics, optics, acoustics, heat, and dynamics. The experiments described are those commonly employed in the elucidation of physiological problems, and they are described in such a way that the book may be used as a hand-book for the laboratory, by means of which the student may work out the experiments for himself. The size of the volume is an indication of the increasing importance of accurate methods in physiological work, and it contrasts remarkably with that by Mr. Power, which includes the whole range of Physiology in about two-thirds the number of similar pages.

The series of red-cover and red-edge volumes which comprise the field commonly included in Physiological Text-books will not be complete without the work by Dr. Ralfe on *Clinical Chemistry*, and that by Professor Jeffrey Bell on *Comparative Physiology and Anatomy*. Armed with this series of monographs, the student should be able to master all the essential points in the subject of Physiology; but it may be questioned whether they will enable him to dispense with the beautifully illustrated eleventh edition of Kirke's *Hand-book* or with the still more enchanting Foster's *Text-book*.

Clinical Notes upon two years' Surgical Work in the Liverpool Royal Infirmary. By W. MITCHELL BANKS. Pp. 190. Printed for the Author. 1884.

Not all the best writing is to be found in elaborate treatises, and not always is most knowledge gained from those who make greatest pretence of instructing us. Our moral is pointed by a perusal of the charming little work before us. The author has taken us into his confidence, and, *currente calamo*, has gossiped over his past two years' work at the Liverpool Infirmary, and shared with us the experiences of

his surgical lifetime. One thought forces itself upon us as we lay down the book; it is this: With what a manly grasp of strong common-sense does the practical provincial English surgeon pick out the possibilities of every theoretical advance in the practice of surgery, ignoring alike the juvenile twaddle of continental thesis-writers and the fame-compelling inventions of enthusiastic embryos in our British metropolis. Whether we believe in all that the writer says or not, every idea comes home to us with a feeling of genuineness which is as novel as it is pleasing. Our pushing writers nowadays have got into a way of editing other people's ideas, and quoting their authors' names in polyglot at the bottoms of their pages; Mr. Banks speaks out for himself from his own lights, and is clear, original, and instructive accordingly.

A Popular Guide to the Use of the Bath Waters, with useful Hints to Visitors; Notes on the Climate of Bath, and its Advantages as a Health Resort and Place of Residence, &c. By J. DOUGLAS KERR, Bachelor of Medicine and Master of Surgery, Double Medalist and Hospital Prizeman Glasgow University, Late Resident Surgeon Glasgow Western Infirmary, Consulting Physician on the Bath Waters. Pp. 60. *Bath Herald* Office. 1884.

A fair notion of the purpose of this work will be gathered from its title page, which we copy *verbatim et literatim*. It is slow work wading through its crude and ungrammatical pages; we give our readers the opportunity of joining us in the few smiles its perusal has called forth. One patient, "after a fortnight's bathing, left *with nearly a clean skin*." The waters are useful in palsy . . . "especially in the *wrist-drop or cholic*, so common among lead-workers and painters." They are also beneficial in "*arthrites*" and in "*leuchorræa*." "Hints on Bathing"—"Having procured a ticket at the office beside the Grand Pump Hotel and selected a bath, the invalid will proceed quietly to undress."

That such a work could emanate from any member of our learned profession is, at least, surprising; but that it should come from one of a class whom we have always recognised as among the most cultured and erudite in our profession—the Bath physicians—is most depressing.

The Medical Chronicle: A Monthly Record of the Progress of the Medical Sciences. Vol. I. Nos. I. & II. Pp. 120. Manchester: J. E. CORNISH. October and November, 1884.

We heartily welcome this most recent evidence of the vigour of thought and work in the provinces. If the *Medical Chronicle* can maintain, even approximately, the high promise of its first numbers, it is evident that it must hold a foremost place among British journals; and if we may judge from the names which appear among its supporters and contributors, we can scarcely doubt that it will easily do this.

It is edited by Drs. Niven and Sinclair, and published under the direction of a committee of nine of the most eminent medical men in Manchester. Original articles occupy about one-fourth of the volume; the rest of the space is filled with very full abstracts and occasional criticisms of current medical literature in Europe. The thoroughness and completeness of these abstracts are certainly nowhere surpassed in British medical journalism, and must have entailed a great deal of labour on the part of those who supplied them. The paper and general get-up are admirable, only we *should* like to have its margins cut. We trust that our contemporary may meet with the success its high excellence undoubtedly merits.

Transactions of the New York Academy of Medicine. Second Series. Vol. I., 1874. Vol. II., 1876. Vol. III., 1883. New York: APPLETON & CO.

These three octavo volumes, printed and bound in the handsome manner which we are so pleasantly familiar with in American publications, contain papers read before the New York Academy of Medicine during the past ten years. Why two years should elapse between the first and second volumes, and seven between the second and third, we cannot say; but we will say that if there were many papers read and not printed which came near the standard of those before us, we should have gladly seen those published also. All the papers in the *Transactions* are worthy of study; some are of a high order of excellence, a few especially so. Among these last we would instance some remarkable papers on "Spectrum Analysis as applied to Medicine," in the first and second volumes. The last volume is the least excellent of the three; we trust that this is no indication of falling off in the work of the Academy.

Tumours of the Ovary, Fallopian Tube, and Broad Ligament. By ALBAN H. G. DORAN, F.R.C.S. Pp. 189. London: SMITH, ELDER & CO. 1884.

Few men are more competent to write a work on ovarian pathology than Mr. Doran. Enjoying unrivalled opportunities at the Samaritan Free Hospital, and making the most of these, he has succeeded in bringing together for examination and study an abundance of material whereon to work out a scientific pathology. His work is ambitious, and justly so. It is evident enough that no one hitherto has aggregated such a mass of facts on his subject as has our author. Not a statement is made nor a conclusion drawn that has not its case or specimen to support it. Everywhere throughout the work there is evidence of careful observation and honest labour in the recording of minutiæ, and thus far it has no rival in the subject on which it treats.

Saying so much, and admitting heartily that it is the best book we have on the subject, we are still disappointed that it is not better. The fault we find with it is that there are too many minutiæ, too many isolated facts, and not enough generalisation. All through the work we are noting individual specimens, reading of individual cases; but rarely do we meet with conclusions built on these units, or when we do they are expressed in such meagre or disjointed terms that they neither impress us nor carry conviction. And there is a want of clear arrangement and method in the first parts of the work, which are most harassing to the reader. The sentences are clear enough; but the paragraphs are confused, and the chapters are simply muddled.

It grieves us to have to write thus of a work of such surpassing excellence generally, and we do so in the hope that a future edition may show an absence of these defects. We could spare the chapters relating to operation, and would be glad to see their space occupied by some philosophical statements of broad pathology such as, we are sure, Mr. Doran could easily produce. Perhaps, when a year or two have passed by, the individual facts will appear less overwhelming, and their generalised bearings will become more prominent.

We reiterate our opinion, that, in spite of its defects, this is the best work on ovarian pathology in existence. We have learnt more from its perusal than we have done from all previous writings on the subject; our only disappointment is, that with his evident capacity to do so, the author has not taught us more.

Medical Extracts.

Muriate of Cocaine, a new local Anæsthetic.

Most remarkable effects are ascribed to this drug, the invention of Dr. Koller, of Vienna. As yet it has had but a limited trial in England, but in America and in Germany its use, in eye operations at least, has now become quite general. There seems to be no doubt as to its extraordinary power of rendering mucous membranes insensible, if we are to judge from the already abundant clinical records in the American and German journals.

We append Messrs. Allen and Hanbury's account of the drug:—

"This salt of cocaine (the alkaloidal principle derived from the leaves of the coca plant—*Erythroxylon Coca*) has lately come into prominent use as a local anæsthetic for delicate mucous surfaces. Whilst it has been successfully tested on the different mucous surfaces of the ear, nose, mouth, urethra, vagina, and rectum, it has been principally used as a means of causing anæsthesia of the eye, and in such application has been successfully used by operators both in Germany and the United States, and by several surgeons in this country. For this especial purpose its use and success were shown in several operations performed at the Ophthalmic Congress recently held in Heidelberg.

"Numerous cases show that the application of a few drops of a 2% or, better, of a 4% solution of muriate of cocaine to the eye causes, within about fifteen minutes from the first application, anæsthesia of the conjunctiva, cornea and surroundings, an anæsthesia so complete that operations for cataract, squint, &c., can be performed during its influence without giving pain to the patient, and without causing any subsequent injury or discomfort to the organ beyond a transient dilatation of the pupil, which is not accompanied by any material interference with sight, and which passes away in a brief time. As previously mentioned, the action of the muriate of cocaine is similar upon the mucous surfaces of other organs, and gives rise to no unpleasant symptoms. It will thus be seen that in this preparation we possess an invaluable agent for producing a safe and satisfactory local anæsthesia of parts intensely sensitive.

"The coca leaves yield only a very minute percentage of the alkaloid, consequently the cost of the material is considerable; but, as the quantity required to produce anæsthesia is very small, its cost becomes a trifling matter compared with the immense benefits derivable from its use.

"The mode of procedure recommended in operations on the eye is as follows:—

"A 2% solution of muriate of cocaine in water is used. Two or three drops are instilled in the usual way on the lower lid. Five minutes afterwards the application is repeated, and after five minutes more the same quantity is again applied. As a rule the anæsthesia is complete at the end of fifteen minutes (often less) from the time of first instillation, and the cornea, and all the surroundings of the eye, may be manipulated at will without causing the slightest pain. The anæthesia generally remains for a period varying from twenty minutes to half an hour, and passes away without causing any discomfort. The pupil becomes dilated about six or eight minutes after the commencement of complete anæsthesia.

"Experience has shown that a 4% solution is much more effective and reliable.

"Cocaine (and its salts) has likewise been strongly recommended for internal use, in small doses, as a powerful nervous stimulant, in full doses as a sedative; and, in the experience of some, has had marked value in preventing craving for alcohol and morphia. Indeed, it has been asserted that the use of morphia may be suddenly stopped without danger to the patient, and without causing the desire for it, if cocaine be carefully administered. On these points, however, further testimony is required to ensure confidence in the use of cocaine."

The Oleate of Copper in Parasitic Skin Diseases.

The recognition of the fact of parasites as the cause of a very large proportion of cutaneous diseases marked a very decided advance in the therapeutics of these affections. The fact having been recognized, the only question which presented itself was the discovery of an agent which should destroy the parasite without causing in the skin itself a greater disturbance than that caused by the insect. It required a number of years for this discovery. Many were the drugs recommended to this end; but the crucial tests of experience showed them nearly all to be indifferently adapted to the end for which they were designed. Possibly, the fact of the ignorance of such a solvent as would most effectually

carry the parasiticide to the exact location of the parasite, and thus insure its actual contact therewith, may have been largely to blame. If this was the case, the desideratum has been very fully supplied in oleic acid. Certainly the salts prepared with this acid, which have been introduced to the profession within a comparatively recent time, have placed in the hands of the profession more effectual means than had been previously enjoyed for the treatment of skin affections of a parasitic nature.

In an article in the August 30th number of the *New York Medical Journal*, Dr. F. Le Sieur Weir, clinical professor of diseases of the skin in the Medico-Chirurgical College of Philadelphia, gives a comprehensive report of the results of his use of the oleate of copper in five hundred cases of parasitic disease of the skin. He prefaces this report with some remarks, with which we are not particularly interested at this time, on the manner of preparing this salt so that its most beneficial action may be secured. The oleate of copper, he remarks, as well as all other oleates, prepared after the manner described, is, to all intents and purposes, a saturated solution; and he regards the existence of any free oleic acid in the preparation as an error which is very apt to complicate the results of the employment of the oleate proper.

Dr. Weir claims to have had exceptional opportunities for testing the curative action of the oleate of copper in parasitic diseases of the skin, and his cases cover no less than seven different forms of such disease; namely: Tinea tonsurans, T. circinata, T. kerion, T. sycosis, T. versicolor, T. favosa and eczema marginatum. In his employment of the various remedies in vogue before the introduction of the oleate of copper, he found that the remedies diminished in their therapeutic activity with the length of time during which they were employed. This objection does not apply to the oleate of copper. His plan of treatment is as follows: From hairy parts he first cuts off the hair close to the skin, over the immediate part affected, and clips an area extending at least one inch beyond the affected surface. The parts are then anointed with fluid cosmoline, or glycerine, or a bread-and-milk poultice is applied, for the purpose of dislodging such scales, crusts, scurf, or actual dirt, as may have accumulated. The ointment of the oleate of copper, of such strength as is suited to the severity of the case, is then rubbed into the diseased patches gently, but thoroughly, so as to secure as complete and rapid absorption as possible. The parts should then be covered or left bare, as the exigencies of the case may dictate. The process of inunction should be repeated

at least twice daily, this being usually amply sufficient. The strength of the ointment varies with the severity of the case, from one drachm to six of the cupric oleate, to the ounce of cosmoline. The results of this treatment, as reported, are certainly very remarkable, few cases requiring its continuance for over two weeks.

It is interesting to note that Dr. Weir regards epilation as a quite unnecessary procedure in the treatment of the diseases above referred to. If additional experience shall show that the use of this preparation may obviate the necessity of epilation, the value of the treatment must be very greatly enhanced. In barber's itch, epilation has heretofore been regarded as an essential step in the treatment; but Dr. Weir emphasizes the statement that he has rarely found it necessary under the use of the oleate of copper, even in this affection. When the disease attacks the eyelashes it is more or less necessary, inasmuch as without it a satisfactory application of the ointment cannot be made.—*The Therapeutic Gazette*, October, 1884.

Operative Opening of Pulmonary Cavities.

Before the late International Medical Congress, Dr. E. Bull (Christiania), in a paper on this subject, laid down the following propositions:—

1. Abscesses of the lung, which can be diagnosed with certainty, and are so situated that they can be opened through the chest-wall, should be treated in the same way as pleural empyema.

2. The condition is the same with regard to limited gangrene of the lung. If several gangrenous foci exist, each one must be treated separately.

3. Echinococci, and 4, foreign bodies in the lung, are to be treated in a similar manner.

5. In bronchiectasis the formation of a pulmonary fistula is indicated only when the accumulation of stagnant matter in large cavities essentially contributes to the deterioration of the patient's condition.

6. In rare cases of tuberculosis, where a large cavity is the predominating condition, the cavity may be laid open with the view of improving the condition of the patient.

7. The operative puncture of a pulmonary fistula is justifiable as a palliative measure.

8. In cases where diagnosis cannot be arrived at, exploratory puncture is certainly of much value; positive as well as negative results may be derived from it.

9. Adhesion of the layers of the pleura, though not to be

insisted on as an absolutely necessary preliminary to the opening of pulmonary cavities.

10. Amyloid degeneration is not an absolute contra-indication to a palliative operation.

11. The use of the thermo-cautery is to be recommended both for the opening of cavities and for the destruction of diseased portions of lung-tissue.—*Medical and Surgical Reporter*, Oct. 11th, 1884.

On the action of Antipyrin.

Dr. Sassjezky has used antipyrin in three cases in doses of 1 to 2 grammes, and comes to the following conclusions:—

1. That it has great temperature reducing power, a temperature of 41° C. in the rectum being brought down to normal by it. He found that one hour after the administration of 2 grammes a fall of from .4 to 1·1° C. was noted, but that this depression reached the maximum three or four hours later, and continued from three to seven hours.

2. Profuse perspiration, reminding one of the action of pilocarpine, comes on two or three hours after the drug is taken in the above dose.

3. The pulse an hour after its administration was slower, after having been to a small extent quickened. It never becomes weaker; on the contrary, somewhat stronger.

4. The temperature of the periphery of the body falls to an extent proportionate to that of the rectum. There were no disagreeable accompaniments observed.

Dr. A. Cahn says that from cases observed in Professor Kussmaul's practice, where antipyrin was used in acute febrile affections, such as typhoid, croupous and catarrhal pneumonia, erysipelas faciei, and pleurisy, depression of temperature always followed its administration, and when the temperature rose again rigor was never seen. In several cases of pneumonia, after large doses of the drug had been taken on the fifth or sixth day of the disease, the temperature remained sub-febrile for some days, although the character of the pulse and respiration showed that the crisis had not taken place. This observation shows that antipyrin not only acts against fever as a symptom, but that it is capable of influencing directly the pathological process. The "antipyrin erythema" which had been previously seen in the cliniques of Breslau and Zürich, was also observed in Strassburg in several cases of typhoid. The erythematory eruption makes its appearance without any subjective symptom, and without any influence on the course of the fever, and consists of round, brownish-red spots, somewhat raised, and which disappear entirely under the pressure of the finger. The extensor

surfaces had more spots on them than the flexor, the back than the chest or abdomen, and the head, palms of the hands, and soles of the feet remained quite free from it. After the administration of the drug was stopped the rash got paler, but reappeared when its use was resumed.—*Deutsche Mediz. Zeitung*, Oct. 27th, 1884.

Tetanus following hypodermic injection of Quinine.

Dr. Bartolozzi, in a communication to the *Gazz. degli Ospitali*, says that he has observed tetanus which terminated fatally follow the hypodermic injection of quinine in two cases out of about two thousand injections, where the drug was thus used during the years 1878 to 1881 in a malarial district. The first case was that of a woman who, whilst showing symptoms of marsh fever, was attacked by severe pleuro-pneumonia; two injections were administered daily for two days—the first into the arm, the second into the thigh—each consisting of—

Morph. Muriat.	...	...	·01 gramme.
Quinine Sulph.	...	...	·30 "
Aquam ad.	...	...	2·00 "

On the thigh a small abscess formed, and the symptoms of tetanus made their appearance after five days.

In the second case, tetanus followed hypodermic injection of quinine, which was administered on account of an attack of malarial fever, in a person suffering from hystero-epilepsy.

Both cases occurred in the spring, with only a short interval between them, at a time when rheumatic affections were prevalent. The author of the paper thinks that some peculiar atmospheric conditions, combined with the pronounced neuropathic character of the patients, taken together, were the predisposing causes of the outbreak of tetanus, which was observed to follow the irritation of the quinine injection.

Dr. Rossi relates another case of a young man, 18 years old, of lymphatic temperament, who came under treatment in November, 1881, with symptoms of pulmonary tuberculosis. In March, 1882, the patient was able to do his daily work again, but in October of that year he became worse; and his temperature being high, and quinine given by the mouth only producing a temporary effect, hydrochlorate of quinine was used hypodermically for some time, and entirely controlled the febrile symptoms. After every injection symptoms of slight irritation manifested themselves, consisting of swelling, circumscribed redness, and pain on pressure, which disappeared after a day or two. In March, 1883, the febrile condition again coming on, the patient wished to have the injections resumed, and for three consecutive days two in-

jections were introduced into the upper and lower arm of each side alternately each day (the hydrochlorate, of the same quality and in the same quantity, being always used). After the last injection symptoms of more severe irritation appeared, the pain of which, however, cataplasmata quickly controlled. Four days after the last injection the pain became worse, and redness and swelling appeared at the site of puncture; suppuration did not result from this, and under suitable treatment the inflammation gradually subsided. On the 29th March the patient felt weak to an extreme degree, and when sitting up in bed he could not keep his head up. On the same day trismus appeared; on the next day pronounced tetanus came on, and during the following night, about thirty hours after the first appearance of symptoms of trismus, the patient died.—*Deutsche Mediz. Zeitung*, Nov. 6th, 1844.

Jequirity.

Dr. S. Pollak, of St. Louis, thus summarises his extensive experience of jequirity:—

1. Jequirity is one of the most interesting and surprising therapeutic agents; very potent, yet limited in its effect.

2. It is the most reliable and prompt remedy in the treatment of trachoma and pannus.

3. The more inveterate the granulations, the more efficient and striking the result.

4. Adults are more amenable to treatment than young individuals.

5. Abrasions, or even ulcerations of the cornea are no bar to its cautious use.

6. Pyorrhœa is no essential factor in the treatment, but a sero-purulent discharge is, as well as the formation of a membrane on the palpebral conjunctiva.

7. Atrophy of the conjunctiva and formation of cicatricial tissue may be exceptional occurrences; they have never been met with by me.

8. A sound bean, and a fresh infusion—not more than 48 hours old, is a *conditio sine qua non;* the deterioration of the infusion is so rapid, that in three days it swarms with bacteria, becomes putrid, fetid and irritating, and unfit for use.

9. The maximum strength of the infusion need not exceed 3 per cent., and a maceration of a few hours suffices to abstract all its medicinal properties. A stronger infusion, and a too frequent application, may induce destructive purulency.

10. Opiates can be dispensed with, for all painful symptoms subside on the discontinuance of the jequirity.—*Amer. Journ. of Ophthalmology*, June 15th, 1884.

Massage.

Dr. Louis Henry, in the *Australian Medical Journal*, Aug. 15th, 1884, gives the following practical instructions for the use of massage:—

1. The most ordinary movement in the carrying out of this treatment is the centripetal. This means the direction of all the strokes towards the centre of the body, or to the heart. The stroking of the legs to be upwards, and of the head and neck downwards, in fact in that direction in which veins and lymphatics are unloaded. The hands of the operator should be anointed with lard, oil, or vaseline, and the palms of the hands must be soft and fleshy, for horny and rough hands are a cause of irritation. The hair on the skin of the patient should be removed by shaving, or cut short with scissors, as constant rubbing and friction very soon sets up an inflammatory condition of the hair bulbs, which causes the patient pain, and compels the operator to desist until the angry appearances have subsided. After each sitting the parts operated on must be enveloped in a bandage or roller of flannel, so as to retain the heat and continue the nervous reaction as long as possible.

Application No. 2 is *massage à friction*. A different movement is made with each hand, the object being to disseminate with one hand what the other forces towards the centre. It is a kind of milking movement. The one hand rubs across and across in a sort of circular movement, while the other makes centripetal parallel strokes. It is a difficult movement, which requires a good deal of practice.

No. 3 is *petrisage*, from *petrir*, to knead. Here you squeeze between the fingers and thumb, raising up the muscles and soft parts—an excellent movement for indurated inflammatory products or hæmorrhagic exudations.

No. 4 is *tapotement*, from *taper*, to strike. Generally the ulnar side of the first and second fingers, or the margin of the hand is used, and applied like the percussion hammer in quick and sharp successive strokes, frequently applied for neuralgic and rheumatic pains, hyperæsthetic and anæsthetic symptoms, and paralysis. The other auxiliary manipulations are active and passive muscular action, electro therapeutics, compression with bandages, baths, forcible rupture of adhesions, &c. After massaging a part we proceed to the passive movement, which consists in the movement of the limb by the masseur, and not till after a prolonged period of treatment will it be found advisable to allow the patient to indulge in independent movements, which are called active. The action of a muscle is like that of a pump, for those

masses or fluids thrown into motion by massage are carried further on by the muscular action. The length of a sitting must depend on the nature of the complaint and the character of the treatment. Sometimes twice daily, from six to ten minutes; sometimes, as in nervous disorders, from half an hour to one hour.

Germ Theory and Heroic Doses.

Any one who has been a close observer must have noticed the growth of therapeutic skepticism in certain quarters during the past couple of decades. This growth has been most rapid among those who have had really the least experience in the treatment of disease, their studies and observations being rather in the line of physiological and pathological research. While the active, working practitioner has been but very slightly affected by these views sought to be promulgated by these authors, it is to be feared that the young practitioners are inclined to carry with them into the active duties of their profession the influence of the teachings of those under whom their medical knowledge was acquired. It is the opinion of the venerable Editor of the *Journal* of the American Medical Association that there is setting in a reaction from this skepticism and expectancy in the treatment of disease, and that the cause of this reaction is largely traceable to the rapid development of the doctrine of germ etiology, and consequent germicide therapeutics. This reaction began with the revival of the antipyretic treatment of fevers, and it is already so great as to justify the fear that patients may be placed in greater danger from the large doses of microbe remedies administered in some cases than from the microbes themselves. Our contemporary very judiciously cautions against practitioners going to too great lengths in this direction. He thinks that the large doses of corrosive sublimate which have latterly been recommended are extremely dangerous, and he advises practitioners against a too ready acceptance of the rose-coloured opinions expressed of the action of this germicide. While, doubtless, there are numerous affections in which the destruction of the bacteria is a consummation devoutly to be wished, the fact should never be lost sight of that there is a danger, while pursuing the parasite, of destroying the patient whom it infects. We would heartily coincide with our contemporary, and admonish our readers that they be not carried away by the weight of authority under which the use of the heroic doses of the bi-chloride of mercury is becoming so popular.—*Therapeutic Gazette*, September, 1884.

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